PHILOSOPHICAL TRANSACTIONS,

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PART I.

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MDCCLXXVII.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the chair, to be given to the authors of such papers, as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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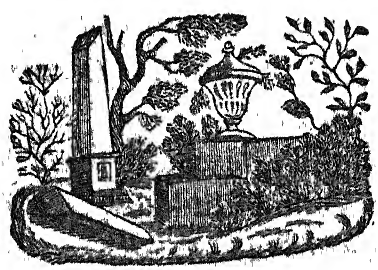

PHILOSOPHICAL TRANSACTIONS.

I. *An Account of a Woman in the Shire of* Rofs *living without Food or Drink. By Dr.* Mackenzie, *Phyfician at* New Tarbat. *Communicated by the Right Honourable* James Stewart Mackenzie, Lord Privy Seal of *Scotland.*

TO SIR JOHN PRINGLE, BART. P. R. S.

Hill-Street,
June 5, 1776.

SIR,

Read Nov. 7, 1776.

I HERE inclofe the cafe of the fafting woman in Rofs-fhire, authenticated by Mr. MAC LEOD the fheriff, and feveral juftices of the peace of that county; that, in cafe you fhall think it worthy to have a place in the Philofophical Tranfactions, it may be inferted therein. I am, with great regard, SIR,

Your moft obedient humble fervant,

J. S. MACKENZIE.

 JANET

JANET MAC LEOD, unmarried, aged thirty-three years and ſome months, daughter of DONALD MAC LEOD, tenant in Croick, in the pariſh of Kincardine, and ſhire of Roſs; in the fifteenth year of her age had a pretty ſharp epileptic fit: ſhe had till then been in perfect health, and continued ſo till about four years thereafter, when ſhe had a ſecond fit, which laſted a whole day and night; and a few days afterwards, ſhe was ſeized with a fever of ſeveral weeks continuance, from which ſhe had a ſlow and very tedious recovery of ſeveral months.

During this period ſhe loſt the natural power of her eye-lids, was under the neceſſity of keeping them open with the fingers of one hand, when ſhe had any thing to do with the other, went out, or wanted to look about her; in every other reſpect ſhe was in health and tolerable ſpirits, only here it may be fit to remark, that ſhe never had the leaſt appearance of the *menſes*, but periodically ſpit up blood in pretty large quantities, and at the ſame time it flowed from the noſe. This vicarious diſcharge, according to her mother's report, happened regularly every month for ſeveral years.

About five years ago, a little before which time the abovementioned periodical diſcharge had diſappeared, ſhe had a ſhort third epileptic fit, which was immediately ſuc-

ceeded by a fever of about a week's continuance, and of which ſhe recovered ſo ſlowly that ſhe had not been out of doors till ſix weeks after the criſis; when, without the knowledge of her parents or any of the family (who were all buſied in the harveſt-field) ſhe ſtole out of the houſe, and bound the corn of a ridge before they obſerved her. On that ſame evening ſhe took to her bed complaining much of her heart and head; and ſince, ſhe has never riſen out of it except when lifted, has ſeldom ſpoken a word, and has had ſo little craving for food that at firſt it was by downright compulſion her parents could get her to take as much as would ſupport a ſucking infant: afterwards ſhe gradually fell off from taking even that ſmall quantity; inſomuch that, at Whitſuntide 1763, ſhe totally refuſed food and drink, and her jaw became ſo faſt locked, that it was with the greateſt difficulty her father was able with a knife or other method to open her teeth ſo as to admit a little thin gruel or whey, and of which ſo much generally run out at the corners of her mouth, that they could not be ſenſible that any of it had been ſwallowed.

Much about this time, that is, about four years ago, they got a bottle of the water from a noted medicinal ſpring in Brea-mar, of which they endeavoured to get her to ſwallow a part, by pouring ſome out of a ſpoon

between her lips (her jaws all the while faſt-locked) but it all run out. With this, however, they rubbed her throat and jaws, and continued the trial to make her ſwallow, rubbing her throat with the water that run out of her mouth for three mornings together. On the third morning during this operation, ſhe cried, Give me more water; when all that remained of the bottle was given her, which ſhe ſwallowed with eaſe. Theſe were the only words ſhe ſpoke for almoſt a year, and ſhe continued to mutter ſome more (which her parents underſtood) for twelve or fourteen days, after which ſhe ſpoke none, and rejected, as formerly, all ſorts of nouriſhment and drink, till ſome time in the month of July 1765, when a ſiſter of hers thought, by ſome ſigns that ſhe made, that ſhe wanted her jaws opened; which her father, not without violence, got done, by putting the handle of a horn-ſpoon between her teeth. She ſaid then intelligibly, Give me a drink; and drank with eaſe, and all at one draught, about an Engliſh pint of water. Her father then aſked her, Why ſhe would not make ſome ſigns, although ſhe could not ſpeak, when ſhe wanted a drink? She anſwered, why ſhould ſhe when ſhe had no deſire. At this period they kept the jaws aſunder with a bit of wood, imagining ſhe got her ſpeech by her jaws being opened, and continued them thus wedged for about twenty days,

 though

though in the firſt four or five days ſhe had wholly loſt the power of utterance. At laſt they removed the wedge, as it gave her uneaſineſs, and made her lips ſore. At this time ſhe was ſenſible of every thing done or ſaid about her; and when her eye-lids were opened for her, ſhe knew every body; and when the neighbours in their viſits would be bemoaning her condition, they could obſerve a tear ſtand in her eye.

In ſome of the attempts to open her jaws, two of the under fore-teeth were forced out; of which opening they often endeavoured to avail themſelves, by putting ſome thin nouriſhing drink into her mouth; but without effect, for it always returned by the corners; and, about a twelvemonth ago, they thought of thruſting a little dough of oatmeal through this gap of the teeth, which ſhe would retain a few ſeconds, and then return with ſomething like a ſtraining to vomit, without one particle going down: nor has the family been ſenſible, though obſerving, of any appearance like that of ſwallowing, for now four years, excepting the ſmall draught of Brea-mar water and the Engliſh pint of common water; and for the laſt three years ſhe has not had any evacuation by ſtool or urine, except that, once or twice a week, ſhe has paſſed a few drops of urine, as the parents expreſs it, about as much as would wet the ſurface

face of a half-penny; and even ſmall as this quantity is, it gives her ſome uneaſineſs till ſhe voids it: for they know all her motions, and when they ſee her thus uneaſy, they carry her to the door of the houſe, where ſhe makes theſe few drops. Nor have they, in all theſe three years, ever diſcovered the ſmalleſt wetting in her bed; in proof of which, notwithſtanding her being ſo long bed-ridden, there has never been the leaſt excoriation, though ſhe never attempts to turn herſelf, or makes any motion with hand, head, or foot, but lies like a log of wood. Her pulſe to-day, which with ſome difficulty I felt (her mother at this time having raiſed her, and ſupported her in her bed) is diſtinct and regular, ſlow, and to the extremeſt degree ſmall. Her countenance is clear and pretty freſh, her features not disfigured nor ſunk; her ſkin feels natural both as to touch and warmth; and to my aſtoniſhment, when I came to examine her body, for I expected to feel a ſkeleton, I found her breaſts round, and prominent, like thoſe of a healthy young woman; her legs, arms, and thighs, not at all emaciated; the *abdomen* ſomewhat tumid, and the muſcles tenſe; her knees bent, and her ham-ſtrings tight as a bow-ſtring; her heels almoſt cloſe to the *nates*. When they ſtruggle with her, to put a little water within her lips, they obſerve ſometimes a dewy ſoftneſs on her ſkin;

ſkin; ſhe ſleeps much, and very quiet; but when awake keeps a conſtant whimpering like a new-born weakly infant, and ſometimes makes an effort to cough. At preſent no degree of ſtrength can force open her jaws. I put the point of my little finger into the gap in her teeth, and found the tongue, as far as I could reach, ſoft and moiſt; as I did with my other fingers the mouth and cheeks quite to the back teeth. She never can remain a moment on her back, but always falls to one ſide or to the other; and when her mother ſat behind her in the bed, and ſupported her while I was examining her body, her head hung down, with her chin cloſe to her breaſt, nor could I with any force move it backwards, the anterior muſcles of the neck being rigid, like a perſon in the *emproſthotonos*, and in this poſture ſhe conſtantly lies.

The above caſe was taken in writing this day, at the diſeaſed woman's bed-ſide, from the mouths of her father and mother, who are known to be people of great veracity, and are under no temptation to deceive; for they neither aſk, expect, or get any thing: their daughter's ſituation is a very great mortification to them, and univerſally known and regretted by all their neighbours. I had along with me, as interpreters (a), Mr. Henry Robertſon, a very diſcreet young gentleman, eldeſt ſon

(a) The family ſpoke only Erſe.

to

to the minifter of the parifh, and David Rofs, at the Craig of Strath-Carron, their neighbour and one of the elders of the parifh, who verified from his own knowledge all that is above related. The prefent fituation and appearances of the patient were carefully examined this 21ft of October, 1767, by Dr. ALEXANDER MACKENZIE, phyfician at New Tarbat; who likewife, in the month of October, 1772, being informed that the patient was recovering and ate and drank, vifited her, and found her condition to be as follows: about a year preceding this laft date, her parents one day returning from their country labours (having left their daughter as for fome years before fixed to her bed) were greatly furprized to find her fitting on her hams, on the fide of the houfe oppofite to her bed-place, fpinning with her mother's diftaff. I afked, whether fhe ever ate or drank? whether fhe had any of the natural evacuations? whether fhe ever fpoke or attempted to fpeak? And was anfwered, that fhe fometimes crumbled a bit of oat or barley cake in the palm of her hand, as if to feed a chicken; that fhe put little crumbs of this into the gap of her teeth, rolled them about for fome time in her mouth, and then fucked out of the palm of her hand a little water, whey, or milk; and this once or twice a day, and even that by compulfion: that the *egefta* were in-

proportion

proportion to the *ingesta*; that she never attempted to speak; that her jaws were still fast-locked, her hamstrings tight as before, and her eyes shut. On my opening her eye-lids I found the eye-balls turned up under the edge of the *os frontis*, her countenance ghastly, her complexion pale, her skin shrivelled and dry, and her whole person rather emaciated; her pulse with the utmost difficulty to be felt. She seemed sensible and tractable in every thing, except in taking food; for, at my request, she went through her different exercises, spinning on the distaff, and crawling about on her hams, by the wall of the house, with the help of her hands: but when she was desired to eat, she shewed the greatest reluctance, and indeed cried before she yielded; and this was no more than, as I have said, to take a few crumbs as to feed a bird, and to suck half a spoonful of milk from the palm of her hand. On the whole, her existence was little less wonderful now than when I first saw her, when she had not swallowed the smallest particle of food for years together. I attributed her thinness and wan complexion, that is the great change of her looks from what I had first seen when fixed to her bed, to her exhausting too much of the *saliva* by spinning flax on the distaff, and therefore recommended her being totally confined to spinning wool; this she does with equal dexterity

as ſhe did the flax. The above was her ſituation in October, 1772; and within theſe eight days I have been told by a neighbour of her father's, that ſhe ſtill continues in the ſame way, without any addition to her ſupport, and without any additional ailment.

New Tarbat,
April 3, 1775.

ALEX. MACKENZIE.

At Croick, the fifteenth Day of June, 1775.

TO authenticate the hiſtory ſet forth in the preceding pages, DONALD MAC LEOD, of Granics eſq. ſheriff depute of Roſs-ſhire, GEORGE MUNRO eſq. of Cuteain, SIMON ROSS eſq. of Gladfield, Captain GEORGE SUTHERLAND of Elphin, all juſtices of the peace; Meſſieurs WILLIAM SMITH preacher of the goſpel, JOHN BARCLAY writer in Tain, HUGH ROSS ſtudent of divinity, and ALEXANDER MAC LEOD, did come to this place, accompanied by the above Dr. ALEXANDER MACKENZIE phyſician at New Tarbat, and after explaining the purport and meaning of the above hiſtory to DONALD MAC LEOD father to JANET MAC LEOD above-mentioned, and to DAVID ROSS elder in the pariſh of Kincardin, who lives in the cloſe neighbourhood of this place, and was one of the doctor's original interpreters; they, to our full ſatisfaction, after

a minute

a minute examination, authenticate all the facts ſet forth in the above account: and, for our further ſatisfaction, we had JANET MAC LEOD brought out before us to the open air, when the doctor diſcovered a very great improvement in her looks and health ſince the period of his having ſeen her laſt, as now ſhe walked tolerably upright, with a little hold by the wall. And notwithſtanding her age, which, upon inquiry, we found to be exactly as ſet forth in the above account, her countenance and looks would have denoted her not to be above twenty years of age at moſt. At preſent, the quantity of food ſhe uſes is not above what would be neceſſary for the ſuſtenance of an infant of two years of age. And we do report, from our knowledge of the above men, and the circumſtances of the caſe, that full faith and credit is to be given to every article of the above hiſtory.

WILLIAM SMITH.	DON^{D}. M^{C} LEOD, SH. DEP.
JOHN BARCLAY, N. P.	GEO. MUNRO, J. P.
HUGH ROSS.	SIMON ROSS, J. P.
ALEX^{R}. M^{C} LEOD.	GEO. SUTHERLAND, J. P.

II. *On the Usefulness of washing and rubbing the Stems of Trees, to promote their Annual Increase. In an Extract of a Letter from Mr.* Marsham *to the Lord Bishop of* Bath *and* Wells.

Read Nov. 14, 1776.

I HAD for several years intended to put in practice the celebrated Dr. HALES advice of washing, with that of Mr. EVELYN of rubbing the stem of a tree, in order to increase its growth; but other avocations prevented me till the last spring: when, as soon as the buds began to swell, I washed my tree round from the ground to the beginning of the head; *viz.* between thirteen and fourteen feet in height. This was done first with water and a stiff shoe-brush, until the tree was quite cleared of the moss and dirt; then I only washed it with a coarse flannel. I repeated the washing three, four, or five times a week, during all the dry time of the spring and the fore-part of the summer; but after the rains were frequent, I very seldom washed. The unwashed tree, whose growth I proposed to compare with it, was (at five feet from the ground) before the last year's increase, 3 ft. 7 in. $\frac{9}{10}$ths; and in the autumn, after the

year's

year's growth was compleated, 3 ft. 9 in. $\frac{1}{10}$th; *viz.* increafe 1 in. $\frac{2}{10}$ths. The wafhed tree was laft fpring 3 ft. 7 in. $\frac{2}{10}$ths, and in the autumn it was 3 ft. 9 in. $\frac{7}{10}$ths; *viz.* increafe 2 in. $\frac{5}{10}$ths, that is, one-tenth of an inch above double the increafe of the unwafhed tree. As the difference was fo great, and as fome unknown accident might have injured the growth of the unwafhed tree, I added the year's increafe of five other beeches of the fame age (*viz.* all that I had meafured), and found the aggregate increafe of the fix unwafhed beeches to be 9 in. $\frac{3}{10}$ths, which, divided by fix, gives one inch and five-tenths and an half for the growth of each tree; fo the gain by wafhing is nine-tenths and an half. To make the experiment fairly, I fixed on two of my largeft beeches, fown in 1741, and tranfplanted into a grove in 1749. The wafhed tree had been, from the firft year, the largeft plant till the year 1767, when its rival became and continued the largeft plant, until I began to wafh the other: therefore I fixed on the lefs thriving tree as the faireft trial. The trees were nearly of the fame height and fhape, fpreading a circle of about fifty feet diameter. I think it neceffary to mention thefe circumftances; for I know by experience, that a fhort and fpreading tree, having ample room, will increafe twice or three times, and perhaps four times as much, as a tall fmall-headed

tree

tree of the ſame age, that ſtands near other trees. Thus my waſhed beech increaſed above ſix times as much as Mr. DRAKE's beautiful beech at Shardeloes, though that tree ſeemed in good health when I ſaw it in 1759 and 1766. But it increaſed only 2 in. $\frac{9}{10}$ths in thoſe ſeven years; which may perhaps be owing to its vaſt height, being ſeventy-four feet and a half to the boughs (as the late knight of the ſhire for Suffolk, Sir JOHN ROUS, told me that Mr. DRAKE had informed him) only ſix feet and four inches round, and having a ſmall head, and little room to ſpread.

Stratton, Oct. 29, 1775.

III. *Discoveries on the Sex of Bees, explaining the Manner in which their Species is propagated; with an Account of the Utility that may be derived from those Discoveries by the actual Application of them to Practice. By Mr.* John Debraw, *Apothecary to* Addenbrook's *Hospital at* Cambridge, *and Member of an Oeconomical Society in the Principality of* Liege *in* Westphalia. *Communicated by the Rev.* Nevil Maskelyne, *B. D. F. R. S. and Astronomer Royal.*

Read Nov. 21, 1776.

THE republic of bees has at all times gained universal esteem and admiration: their culture, an object so worthy of our attention, has attracted and still does engage that of many of the learned, and has arrived at a considerable degree of improvement of late years; but their mode of propagating their species seems to this day to have baffled the ingenuity of ages in their attempts to discover it. The most skilful naturalists have been strangely misled in their opinion, that the bees, as well as the other tribes of animals, are perpetuated by copulation; though they acknowledge

acknowledge that they have never been able to detect them in the act.

PLINY, who was likewife of the fame opinion, that in this particular they do not differ from other animals, obferves, "*Apium coitus vifus eft nunquam.*" SWAMMERDAM, that fagacious obferver, having never been able to difcover it, entertained a notion, that the female or queen bee was fecundated without copulation; that it was fufficient for her to be near the males; that a vivifying *aura*, exhaling from the body of the males, and abforbed by the female, might impregnate her eggs. At laft the incomparable REAUMUR thought he had in a great meafure removed the veil, and brought their manner of generating nearly to a proof. This part of phyfics has been the principal object of my refearches for feveral years paft, having been infenfibly engaged in it by the pleafure I took in fo curious an inquiry; and although this purfuit has been attended with more difficulties and embaraffments than can be well imagined, I have not been difcouraged, and have carefully avoided launching into conjectures. To introduce a new fyftem in the doctrine of bees, which in a great meafure contradicts all former received opinions, requires, previous to its appearance, every fanction the various experiments, fuccefsfully repeated, can poffibly give it. The refults of thofe experiments,

ments, made all in glaſs-hives, which carry with them an entire evidence, afford ſufficient reaſons to aſſert, that bees belong to that claſs of animals among which, although they have ſexes, a true copulation cannot be proved; and that their *ova*, like the ſpawn of fiſhes, moſt probably owe their fecundation to an impregnation from the males, as will appear in the ſequel of this narrative.

I am not a little pleaſed to find that the celebrated MARALDI had ſuch a notion, and I lament his neglecting to confirm it. He ſays, in his *Obſervations upon Bees*, in the Hiſtory of the Academy of Sciences for the year 1712, p. 332: *Nous n'avons pû découvrir juſqu' à preſent de quelle maniere ſe fait cette fécondation, ſi c'eſt dans le corps de la femelle, ou bien ſi c'eſt à la maniere des poiſſons, aprés que la femelle a poſé ſes œufs: la matiere blanchâtre dont l'œuf eſt environné au fond de l'alvéole peu de temps aprés ſa naiſſance, ſemble conforme à la derniere opinion, auſſi-bien que les remarques faites pluſieurs fois d'un grand nombre d'œufs qui ſont reſtés inféconds au fond de l'alvéole autour deſquels nous n'avons point vû cette matiere.* "We never yet were able to diſcover in what manner "this fecundation is performed; whether it is in the body "of the female, or whether it is after the manner of "fiſhes, after the female or queen-bee has depoſited "her eggs: that liquid whitiſh ſubſtance, with which

" each egg is surrounded at the bottom of the cell a " little while after its being laid, seemingly establishing " this last opinion, as well as the frequent remarks made " of a great number of eggs remaining barren in the cell, " round which we could not see the above-mentioned " whitish substance."

This ingenious naturalist, by a nice examination of the structure of the drones, had, as well as SWAMMERDAM, discovered some resemblances to the male organs of generation; and from thence conjectured, they were the males of the bee-insect; but he owns, with the rest, that he never could discover them in the act of copulation.

Having stood the trials of so many prying eyes in every age, the bees, as has been observed by an ingenious author, had gained the character of an inviolable chastity, till REAUMUR blasted their reputation. He makes the queen no better than a MESSALINA [a]; though he could see no more than what would raise a mere jealousy or generate suspicions.

In order to be the better understood in the relation of my own experiments on the fecundation of bees, I here premise the outlines of the opinions adopted by the above-mentioned naturalists on that head. They assert that the

[a] Vid. JUVENAL, Sat. vi. ver. 128.

queen

queen is the only female in the hive, and the mother of the next generation; that the drones are the males by which ſhe is fecundated; and that the working bees, or bees that collect wax on the flowers, that knead it and form from it the combs and cells which they afterwards fill with honey, are of neither ſex.

But of late Mr. SCHIRACH, a German naturaliſt, has given us a very different view of the claſſes that conſtitute the republic of bees, in an ingenious publication in his own language, under the title of *The Natural Hiſtory of the Queen of the Bees*, which has been ſince tranſlated into French; an account of which has been given in the Monthly Review; from which I beg leave to relate the author's doctrine with regard to the working-bees only; the quality and functions of the drones being points which do not appear to be yet ſettled by Mr. SCHIRACH himſelf. He affirms, that all the common bees are females in diſguiſe, in which the organs that diſtinguiſh the ſex, and particularly the *ovaria*, are obliterated, or at leaſt, through their exceſſive minuteneſs, have not yet been obſerved: that every one of thoſe bees in the earlier period of its exiſtence is capable of becoming a queen-bee, if the whole community ſhould think proper to nurſe it in a particular manner, and raiſe it to that rank. In

ſhort, that the queen-bee lays only two kinds of eggs; *viz.* thoſe that are to produce the drones, and thoſe from which the working-bees are to proceed.

The trials made by Mr. SCHIRACH ſeem to evince the truth of his concluſions in the moſt ſatisfactory manner, ſingular as they appear to be at firſt ſight; and indeed in my own judgement, from the conſtant happy reſult of my numerous experiments, which I began near two years before Mr. SCHIRACH's publication, and repeated every ſeaſon ſince, I am enabled to pronounce on their reality.

Chance I own befriended me in that diſcovery, whilſt I was moſt anxiouſly endeavouring to aſcertain the uſe of drones. It was in the ſpring of the year 1770, that I for the firſt time diſcovered what MARALDI had only conjectured, I mean the impregnation of the eggs by the males, and that I was made acquainted with the difference of ſize in the drones or males obſerved by MARALDI in his *Obſervations upon Bees*, inſerted in the Hiſtory of the Royal Academy of Sciences for the year 1712, p. 333. in theſe words:

Nous avons trouvé depuis peu une grande quantité de bourdons, beaucoup plus petits que ceux que nous avions remarqué auparavant, et qui ne ſurpaſſent point la grandeur des petites abeilles; de ſorte qu'il n'auroit pas été aiſé

de

de les distinguer dans cette ruche de abeilles ordinaires, sans le grand nombre que nous y en avons trouvé. Il se pourroit bien faire que dans les ruches où l'on n'a pas trouvé de gros bourdons, il y en eût de ces petits, et qu'ils y aient été confondus avec le reste des abeilles, lorsque nous ne savions pas encore qu'il y en eût de cette taille. "We have of "late found a great quantity of drones much smaller "than those we had formerly observed, and which do not "exceed in size the common bees; so that it would not "have been easy to distinguish them in that hive from "the common bees, had not the quantity of them been "very considerable. It might certainly have happened "that in those hives, where we have not been able to "discover large drones, there were a great number of "those little ones, which may have been intermixed "among common bees when we were yet ignorant that "any such small drones were existing."

REAUMUR himself, p. 591. of his Natural History of Insects, says, "We have likewise found drones that were "no bigger than the common bees."

They have notwithstanding escaped the observation of Mr. SCHIRACH, and of his friend Mr. HATTORF member of an Academy in Lusatia, who, in a memoir he presented in the year 1769, annihilates entirely the use of drones in a hive; and advances this singular opinion,

opinion, that the queen-bee of a hive lays eggs which produce young ones, without having any communication with the drones. For what purpose should wise nature then have furnished the drones with that large quantity of seminal liquor? To what use so large an apparatus of fecundating organs, so well described by REAUMUR and MARALDI?

But I beg leave to remark, that those gentlemen seem to have drawn too hasty conclusions from their experiments, in rejecting the drones as bearing no share in the propagation of those insects. Their observations, that hives are peopled at a time of the year when there are no drones in being, is no ways conclusive; as it is evident, that they had seen none but drones of a large size, their silence on the difference in the size of them justifying my remark. But to resume the narrative of my experiments: I had watched my glass-hives (b) with indefatigable attention from the moment the bees, among which I had taken care to leave a large number of drones, were put into them, to the time of the queen laying her eggs, which generally happens the fourth or fifth day. I observed the first or second day (always before the third) from the time the eggs are placed in the cells, that a great number

(b) Glass-hives were used in preference to boxes, for a purpose too obvious to need explaining.

of

of bees, faſtening themſelves to one another, hung down in the form of a curtain from the top to the bottom of the hive, in a ſimilar manner they had done before at the time the queen depoſited her eggs; an operation which (if we may conjecture at the inſtincts of inſects) ſeems contrived to hide what is tranſacting: be that as it will, it anſwered the purpoſe of informing me that ſomething was going forward. In fact, I preſently after perceived ſeveral bees, the ſize of which through this thick veil (if I may ſo expreſs myſelf) I could not rightly diſtinguiſh, inſerting the poſterior part of their bodies each into a cell, and ſinking into it, where they continued but a little while. After they had retired, I ſaw plainly with the naked eye a ſmall quantity of a whitiſh liquor left in the angle of the baſis of each cell, containing an egg: it was leſs liquid than honey, and had no ſweet taſte at all. Within a day after, I found this liquor abſorbed into the embrio, which on the fourth day is converted into a ſmall worm, to which the working-bees bring a little honey for nouriſhment, during the firſt eight or ten days after its birth. After that time they ceaſe to feed them; for they ſhut up the cells, where theſe embrios continue incloſed for ten days more, during which time they undergo various changes too tedious here to deſcribe.

To

To evince the reality of this obſervation, and to prove that the eggs are fecundated by the males, and that their preſence is neceſſary at the time of breeding, I proceeded to the next experiments. They conſiſted in leaving in a hive the queen with only the common bees, without any drones, to ſee whether the eggs ſhe laid would be prolific. I accordingly took a ſwarm, ſhook all the bees into a tub of water, and left them in it till they were quite ſenſeleſs, which gave me an opportunity to diſtinguiſh the drones without any danger of being ſtung. After I had recovered the working-bees and their queen from the ſtate they were in, by ſpreading them on brown paper in the ſun, I replaced them in a glaſs-hive, where they ſoon began to work as uſual: the queen laid eggs, which I little ſuſpected to be impregnated, as I thought I had ſeparated all the drones or males, and therefore omitted watching the bees; but at the end of twenty days (the uſual time of their hatching) I found to my ſurprize ſome of the eggs hatched into bees, others withered away, and ſeveralof them covered with honey. I immediately inferred that ſome of the males, having eſcaped my notice, had impregnated only part of the eggs; but, in order to convince myſelf of the truth of my ſuppoſition, I thought it neceſſary to take away all the brood-comb that was in the hive, in order to oblige

the bees to provide a freſh quantity, being fully determined to watch narrowly their motions after new eggs ſhould be depoſited in the cells. This was done accordingly, and at laſt the myſtery was unravelled. On the ſecond day after the eggs were placed in the cells, I perceived the ſame operation which I have related in a former experiment; I mean, the bees hung down in the form of a curtain, while others thruſt the poſterior part of their body into the cells: I then introduced my hand into the hive, broke off a piece of the comb containing two of thoſe inſects, and kept them for examination. I found in neither of them any ſting (a circumſtance peculiar to drones only) and upon diſſection, by the help of a DOLLOND's microſcope, diſcovered in them the four cylindrical bodies, which contain the glutinous liquor of a whitiſh colour, obſerved by MARALDI in the large drones.

Having till then never obſerved any difference in the ſize of drones, I immediately peruſed the Memoirs on Bees publiſhed by Meſſ. MARALDI and REAUMUR, and found that they had remarked it frequently. I have inſerted in a preceding page the ſubſtance of their obſervations on that head, as taken from their writings. The reaſon of that difference muſt I doubt be placed amongſt other *arcana* of nature. I found myſelf therefore under a neceſſity in my next experiments to be more particular

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in deftroying the males, even thofe which might be fufpected to be fuch.

I once more immerfed all the fame bees in water; and, when they appeared to be in a fenfelefs ftate, I gently preffed every one of them between my fingers, in order to diftinguifh thofe armed with ftings from thofe that had none, which laft I might fufpect to be males. Of thefe I found fifty-feven, exactly of the fize of common bees, yielding a little whitifh liquor on being preffed between the fingers. I killed every one, and replaced the fwarm in a glafs-hive, where they immediately applied again to the work of making cells; and on the fourth or fifth day, very early in the morning, I had the pleafure to fee the queen-bee depofiting her eggs in thofe cells, which fhe did by placing the pofterior part of her body in each of them. I continued on the watch moft part of the enfuing days, but could difcover nothing of what I had feen before.

The eggs, after the fourth day, inftead of changing in the manner of caterpillars, were found in the fame ftate they were in the firft day, except that fome of them were covered with honey. But a very fingular event happened the next day about noon: all the bees left their own hive, and were feen attempting to get into a neighbouring common hive, on the ftool of which I found their queen dead,

having no doubt been ſlain in the engagement. The manner in which I account for this event is as follows: the great deſire of perpetuating their ſpecies, which is moſt obſervable in theſe inſects, and to which end the concurrence of the males ſeems ſo abſolutely neceſſary, made them deſert their own habitation where no males were left, in order to fix their reſidence in a new one, in which, there being a good ſtock of males, they might the better accompliſh their purpoſe. If this does not yet eſtabliſh the reader's faith of the neceſſity of the males bearing a ſhare in the fecundation of the *ova*, the next experiment cannot I preſume fail to convince him.

I took the brood-comb which, as I obſerved before, had not been impregnated; I divided it into two parts; one I placed under a glaſs-bell N° 1. with honey-comb for the bees' food; I took care to leave a queen, but no drones, among the common bees I confined in it. The other piece of brood-comb I placed under another glaſs-bell N° 2. with a few drones, a queen, and a number of common bees proportioned to the ſize of the glaſs; the reſt I diſpoſed of as before. The reſult was, that in the glaſs N° 1. no impregnation happened; the eggs remained in the ſame ſtate they were in when put into the glaſs; and, upon giving the bees their liberty on the ſeventh day, they all flew away, as was found to be the caſe in the for-

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mer experiment: whereas in the glaſs N° 2. I ſaw, the very day after the bees had been put under it, the impregnation of the eggs by the drones in every cell containing eggs; the bees did not leave their hive on receiving their liberty; and, in the courſe of twenty days, every egg underwent all the above-mentioned neceſſary changes, and formed a pretty numerous young colony, in which I was not a little ſtartled to find two queens.

Fully ſatisfied concerning the impregnation of the eggs by the males, I deſiſted for the preſent from any further experiments on that head, being exceedingly anxious to endeavour to account for the preſence of this new queen.

I conjectured that either two queens, inſtead of one, muſt have been left among the bees I had placed under that glaſs; or elſe that the bees could, by ſome particular means of their own, transform a common ſubject into a queen.

In order to put this to the teſt, I repeated the experiment with ſome variation. I got four glaſs-hives blown flat, which I thought preferable to the bell-ſhaped ones I had uſed before, as I could with thoſe better examine what was going forward. I took a large brood-comb from an old hive, and, after having divided it into ſeveral pieces, I put ſome of them, containing eggs, worms, and nymphs, with food, *viz.* honey &c. under each of the

glaſſes;

glaſſes; and confined within each a ſufficient number of common bees, among which I left ſome drones, but took care that there ſhould be no queen.

The bees finding themſelves without a queen, made a ſtrange buzzing noiſe, which laſted near two days; at the end of which they ſettled and betook themſelves to work: on the fourth day I perceived in each hive the beginning of a royal cell, *a certain indication that one of the incloſed worms would ſoon be converted into a queen.* The conſtruction of the royal cell being nearly accompliſhed, I ventured to leave an opening for the bees to get out, and found that they returned as regularly as they do in common hives, and ſhewed no inclination to deſert their habitation. But, to be brief, at the end of twenty days, I obſerved four young queens among the new progeny.

On relating the reſult of theſe experiments to a member of this univerſity, well converſant in the natural hiſtory of bees, he deemed it neceſſary, that they ſhould be repeated, in order the better to eſtabliſh the truth of a fact ſeemingly ſo improbable, that the eggs deſtined by nature to produce neutral or common bees, ſhould be transformed into females or queens. He ſtarted an objection to me, which by the publication of Mr. SCHIRACH appearing a little time after, ſeems to have been pointed out to that author alſo by Mr. WITHELMI,

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his brother-in-law, namely, that the queen-bee of a hive, befides the eggs which fhe depofits in the royal cells, might alfo have laid royal or female eggs either in the common cells, or indifcriminately throughout the different parts of the hive. He further fuppofed, that in the pieces of brood-comb, which had been fuccefsfully employed in the laft experiments for the production of a queen, it had conftantly happened, that one or more of thefe royal eggs, or rather the worms proceeding from them, had been contained.

But the force of his objection was removed foon after by the fame fuccefs having attended a number of other experiments which I fince made, an account of which would take up too much room here; and this gentleman, together with Mr. SCHIRACH's brother-in-law, was at laft brought to admit, that the working-bees are invefted with a power of raifing a common fubject to the throne, when the community ftands in need of a queen; and that accordingly every worm of the hive is capable, under certain circumftances, of becoming the mother of a generation: that it owes its metamorphofis into a queen, partly to the extraordinary fize of the cell, and its particular pofition in it; but principally to a certain nourifhment appropriated to the occafion, and carefully adminiftered to it by the working-bees while it is in the worm-ftate,

by

by which, and poſſibly other means as yet unknown, the developement and expanſion of the germ of the female organs, previouſly exiſting in the embryos, is effected, and thoſe differences in its form and ſize are produced, which afterwards ſo remarkably diſtinguiſh the queen from the common working-bees. And finally it appears evident, from the experiments made by Mr. SCHIRACH and myſelf, that the received opinion, that the queen lays a particular kind of eggs, appropriated to the production of other queens, is erroneous. I am not a little flattered with the ſimilarity of my diſcoveries with thoſe of the ingenious German naturaliſt, in proving the ſex of the common bees; although we ſo widely differ in what relates to the uſe of the males, whom, as we have ſeen before, he imagines to be quite uſeleſs. I am alſo not a little pleaſed to find, that our experiments on the production of a queen from a common embryo agree ſo well.

I ſhall now beg leave to point out the advantage that may accrue to the public from theſe obſervations, which is that of forming artificial ſwarms or new colonies; or in other words, of furniſhing the means to bring on a numerous increaſe of thoſe uſeful inſects: an object of ſome importance to this kingdom, as being the only means to prevent the annual exportation of conſiderable ſums in the purchaſe of wax, a great quantity of

of which is left every feafon for want of keeping up a fufficient ftock of bees to collect it.

The practice of this new art, Mr. SCHIRACH tells us, has already extended itfelf through Upper Lufatia, the Palatinate, Bohemia, Bavaria, Silefia, and even in Poland. In fome of thofe countries it has excited the attention and patronage of government; and even the Emprefs of Ruffia has thought it of fuch importance, that fhe has fent a perfon to Klein Bautzen, to be inftructed in the general principles, and learn all the *minutiæ* of this new art.

The narrow limits of this paper do not permit me here to give an account of Mr. SCHIRACH's ingenious obfervations. I beg leave to refer the curious reader to the work itfelf, which, with the reviewers, I wifh was tranflated into the Englifh language, as it contains many particulars highly deferving the notice of the fpeculative naturalift, as well as of thofe who cultivate bees either for profit or amufement.

IV. *An Account of a Portrait of* Copernicus, *presented to the* Royal Society *by Dr.* Wolf *of* Dantzick: *extracted from a Letter of his to Mr.* Magellan, *F. R. S.*

DEAR SIR, Dantzick, April 7, 1776.

Read Dec. 7, 1776.

THE captain who will deliver this to you, will alſo put into your hands a copy of an original portrait of the famous COPERNICUS, which I beg you will preſent to the Royal Society, as a teſtimony of my devotion and attachment to that reſpectable body. The original, from which it is copied with the greateſt accuracy, is in the poſſeſſion of the Chamberlain HUSSARZEWSKI,

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Avis touchant un Portrait de Copernic, *préſenté à la* Societé Royale, *par M. le Dr.* Wolf *de* Danzic: *extrait d'une Lettre du dit Dr. à M. de* Magellan, *Membre de la Soc. R.*

Monſieur et très cher ami, Danzic, le 7 Avril, 1776.

LE capitaine qui vous délivrera celle-ci, vous apportera en même tems la copie d'un portrait du fameux COPERNIC, que je vous prie de préſenter à votre illuſtre Societé Royale, comme un temoignage de mon dévouement et reſpect pour cet illuſtre Corps. Le poſſeſſeur de l'original, dont le portrait fut copié avec la plus grand attention, eſt M. le Chambellan HUSSARZEWSKI. Il a

 déja

SARZEWSKI, who has already refufed one hundred ducats for it, and will not part with it at any rate during his life, but intends to bequeath it me after his death; for which reafon there is no probability of my ever poffeffing it, as he is likely to furvive me many years. We have a portrait of COPERNICUS in the great church at Thorn in a kind of maufoleum, erected about thirty years after the death of that great man, by a phyfician of that town, who is faid to have been one of his relations.

HARTKNOCH has inferted a print taken from this portrait in his Chronicles of Pruffia. Our original has been compared with that of the maufoleum, and the features of the face are found to be perfectly fimilar, but there is a great difference in the drefs. That at Thorn reprefents him kneeling before an altar, in the attitude of a prieft officiating; in ours he is cloathed in fur, with his hair more

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déja refufé 100 ducats, et ne le cédera pas fa vie durant pour aucun prix : mais il me le veut léguer après fa morte. C'eft à dire très probablement après la mienne: de forte qu'il n'y a rien à efpérer de ce coté-ci. Nous avons un portrait de COPERNIC dans la grande Eglife de Thorn, dans une efpéce de maufolée, érigé une trentaine d'années après la mort de cet homme célebre, par un médecin de la ville, qu'on dit avoir été un de fes parents.

HARTKNOCH a fait graver ce portrait, et l'a inferé dans fa Chronique de la Pruffe. Notre original a été comparé fur le lieu avec celui du maufolée, et on a trouvé les traits du vifage abfolument les mêmes: mais l'habillement eft très different. Celui de Thorn le reprefente devant l'autel en fonction de prêtre. Dans

more carefully dreſſed, and as it were in a habit of ceremony. The painter of it was certainly one of the old Italians, as will appear by comparing it with other works of thoſe maſters; for inſtance, it is known that the painters of thoſe times, and even RAPHAEL, never gave to the eyes that brightneſs which the moſt indifferent artiſts within this century never fail to expreſs in their portraits: not but what the ſerene and ſeemingly inanimated countenances of the former artiſts came nearer to nature than the ſparkling eyes which are now all the faſhion. This however is a proof that the portrait is at leaſt one hundred and fifty years old; the inſcription ſhews that the painter was an Italian; and it muſt further be obſerved, that it is now two centuries ſince they left off painting on wood.

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Dans le notre il eſt habillé en péliſſe, avec les cheveux de la tête plus ſoigneuſement arrangés et coupés, comme en habit de parade. Le pinceau eſt ſûrement Italien du vieux tems en queſtion, comme on trouvera en regardant d'autres de ce tems et les comparant avec celui-ci. Par exemple, on ſait que les peintres dans ce vieux tems, même RAPHAEL, ne ſavoient pas encore donner aux yeux cette vivacité, que le plus médiocre peintre depuis plus d'un ſiecle, ne manque pas de donner à ſes peintures. Je ne dis pas que les vieux peintres n'approchent pas plus de la nature avec leurs yeux tranquiles ſans feu; mais ils déplaiſent à nôtre ſiecle, qui ne veut que des yeux petillants. Au moins c'eſt la marque d'une ancienneté plus grande qu'un ſiecle et demi. L'inſcription fait voir que le peintre etoit Italien. Il y a deux ſiecles qu'on a ceſſé de peindre ſur du bois.

 L'hiſtoire

The hiſtory of this portrait is as follows. It was formerly in the collection of Saxe Gotha, where it was always conſidered as an original, which is even ſaid to appear from the archives of that court, and is the more probable, as the prince-biſhop of Warmia, who obtained it from the late duke of Saxe Gotha, was too good a connoiſſeur and too cautious to be deceived in this reſpect. That biſhop being at Gotha in the year 1735, obſerved this portrait in the gallery of that palace; the proofs that were produced of its authenticity made him very deſirous to acquire it. He at length obtained it by a kind of theft which it was neceſſary to commit on the cathedral of Warmia, in which there was a very old portrait of one of the anceſtors of the dukes of Saxe Gotha, who had been biſhop of that ſee, and whoſe picture was wanting in

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L'hiſtoire de notre original eſt la ſuivante. Il a été conſervé dans la collection des ducs de Saxe Gotha, dans le chateau du même nom, toujours avoué comme un véritable original: on prétend même prouvé comme tel par les archives; ce qui eſt fort croyable, vû que le prince evêque de Warmie, qui l'a obtenû du duc defunt, etoit trop circonſpect et trop bon connoiſſeur pour s'en laiſſer impoſer. Enfin c'eſt après les paroles du prince evêque, que je fais cette aſſertion. Ce fut environ l'année 1735 qu' etant à Gotha, il remarqua ce portrait dans la gallerie du duc. Les preuves qu'on lui fit de ſon authenticité, augmenterent ſon envie de le poſſéder. Il l'obtint enfin par un vol qu'il fallut faire à la Cathédrale de Warmie, où ſe trouvoit un très ancien portrait d'un des ancétres du duc de Saxe Gotha, qui etoit jadis evêque de Warmie, et dont le portrait manquoit dans la collection des portraits de ſa famille. On fit donc un échange

in the duke's collection of the portraits of his family. An exchange was accordingly made of the two originals, and the biſhop has ſince bequeathed that of COPERNICUS to his favourite Mr. HUSSARZEWSKI.

The copy I herewith ſend you is very exact in every reſpect, except that I have glued three wooden liſts on the back of the board to prevent its warping. After my copy was taken, the proprietor of the original thought proper to have it repaired and varniſhed, and they are now ſo much alike that it is ſcarce poſſible to diſcern any difference. The name of my painter is LORMAN of Berlin, an artiſt of ſome reputation.

échange des deux originaux. Le prince evêque GRABOWSKI enfin a laiſſé celui de COPERNIC à ſon favori M. HUSSARZEWSKI.

La copie eſt abſolument la même que l'original: excepté que j'ai fait coller trois règles ſur le derriere de la table pour empêcher ſon courbiſſement. Depuis que ma copie a été tirée, M. HUSSARZEWSKI, croiant bien faire, a fait renouveller ſon original, c'eſt à dire, y mettre du vernis: et actuellement il eſt ſi bien le même que la copie, qu'on peut a peine s'appercevoir de la moindre différence. Mon peintre eſt M. LORMAN de Berlin, aſſez célèbre dans ſon art.

V. *An Account of a Journey into* Africa *from the Cape of* Good-Hope, *and a Deſcription of a new Species of* Cuckow. *By Dr.* Andreas Sparrman, *of the Royal Academy of* Stockholm, *in a Letter to Dr.* John Reinhold Forſter, *F. R. S.*

DEAR SIR, Gottenburg, Sept. 16, 1776.

Read Dec. 19, 1776.

BEING returned to my native country after an abſence of five years from it, I will endeavour to give you a ſhort account of my expedition into Africa, which I undertook ſoon after parting with you at the Cape of Good-Hope. The voyage round the world, of which I ſhared the perils and pleaſures with you, had only made me more eager to continue my rambles in queſt of new diſcoveries. I ſet out therefore from the neighbourhood of the Cape-town in the beginning of Auguſt 1775, with no other company than the ſon of the Dutch lieutenant EMELMAN, who had formerly accompanied my learned friend Dr. THUNBERG on a ſimilar journey, and ſome Hottentots who took care of my oxen.

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The firſt misfortune I met with was the loſs of the thermometer which you had left me, and which broke before I had reached the hot-baths. This was only a prelude to greater diſtreſſes. The drought was ſo violent this year, that the like had not been experienced in the colony within the memory of man, and it obliged the inhabitants to leave their country-ſeats. A great part of their cattle periſhed for want of graſs and water, and I have frequently ſuffered the moſt raging thirſt in the hot deſarts which I traverſed; but I was too well ſeaſoned during the voyage to dread the hardſhips of a ſcanty ſubſiſtence, the fatigues of travelling, or the power of the climate. The moſt ſenſible misfortune which the dry ſeaſon brought along with it, was the deſolation of the vegetable kingdom. Far from being ſo fortunate as Dr. THUNBERG, who has added above a thouſand ſpecies to the *Flora Capenſis*, I found every thing burnt up, and only in the thickeſt foreſts met with ſome perennial plants which were new to me, and which, upon a reviſal of that gentleman's herbal, I believe are likewiſe unknown to him. Of theſe I propoſe to ſend you ſpecimens as ſoon as I can find time to bring my collection into ſome kind of order. On the other hand, I have been fortunate with animals, and eſpecially in the claſs of quadrupeds. I ſhall not ſpeak of lions and other beaſts

of

of prey, which I have frequently ſeen in broad day-light, and heard roaring about me at night, though they never ventured to attack our cattle. But it was chiefly among the antelopes and animals of that ſort that I hunted. Mr. EMELMAN and myſelf, with nine hottentots, a waggon drawn by ſeveral pairs of oxen, and ſeveral hunting horſes, happily traverſed a deſart of fifty miles, where we had greater ſport than any German prince could ever boaſt of. On that route I penetrated farther into the country than any of my predeceſſors, having gone one hundred miles beyond the laſt Chriſtian's or Dutchman's hut, into the diſtrict of the Yellow or (as they are vulgarly called) Chineſe Hottentots.

The great buffaloes which inhabit the wilds of Africa, do not appear to me to differ in any reſpect from the North American *Biſon*, although I have ſeen great numbers of them. I have likewiſe found a ſpecies of pole-cat on that continent which LINNÆUS calls *Viverra Putorius*, contrary to M. DE BUFFON'S opinion, who ſeems to confine this animal and its ſpecies to America. By the ſea-ſide I was fortunate enough to catch a *Manatee* alive, notwithſtanding the difficulty which muſt attend the capture of ſuch an unweildy animal. There I likewiſe ſaw ſome iſlands, on which I was told an Engliſh ſhip had been loſt. Theſe I ſuſpected at firſt to be the *Doddingtons*; but afterwards

afterwards had reaſon to doubt it, thoſe iſlands being ſuppoſed to lie in a more ſoutherly latitude.

I have had opportunities of making many curious and valuable Obſervations relative to the different tribes of Hottentots, their oeconomy, hunting-matches, and other cuſtoms; an account of which, together with ſome remarks on the natural hiſtory of the elephant, the rhinoceros, and other animals, I intend to prepare for the preſs. I am poſſeſſed of an accurate map of that part of Africa which I have viſited, containing all the hills, together with the ſmalleſt rivulets, as far as the Bay de la Goa, which, I think, will be a great addition to the work. I only regret that I was not able to draw the objects of natural hiſtory, and have an hundred times wiſhed that your ſon had been with me for this purpoſe.

As I had been upwards of nine months on this journey, at my return to the Cape I found that my acquaintance had given up all hopes of ſeeing me again, having had no tidings of me for ſo long a ſpace of time. Notwithſtanding the many dangers to which I had been expoſed on this expedition, I aſſure you I was greatly tempted to ſtay another year, in hopes of being more ſucceſsful in botanical diſcoveries. However, the proſpect of ſecuring the ſpoils which I had collected, prevailed on me to relinquiſh that ſcheme. Indeed I little thought at that time that the greateſt

 danger

danger awaited my collection in Sweden. A few days ago a great part of it has been damaged here by fire, which has been particularly fatal to my ſtuffed birds, having deſtroyed ſome which were not yet deſcribed.

As I am well acquainted with the pleaſure which every new diſcovery in the hiſtory of nature gives you, I take this early opportunity of expreſſing the readineſs with which I wiſh to contribute to your ſatisfaction, and have ſubjoined to this letter an account of a curious bird, a ſpecies of Cuckow, which I have ſaved out of the fire. I only beg that you will conſider it as an earneſt of more important communications, as ſoon as the hurry of my affairs will permit me to bring my papers into order. In the mean time if you ſhould think that account, and the annexed drawing, worthy the attention of the Royal Society, I ſhould be greatly flattered if you would do me the honour to lay it before that learned Body.

With the greateſt eſteem I remain, &c.

THE

THE HISTORY OF THE HONEY-GUIDE, OR CUCULUS INDICATOR.

THIS curious fpecies of Cuckow is found at a confiderable diftance from the Cape of Good-Hope, in the interior parts of Africa, being entirely unknown at that fettlement. The firft place where I heard of it was in a wood, called the *Groot Vaader's Bofch*, the Grand Father's Wood, fituated in a defart near the river which the Hottentots call *T'kaut'kai*. The Dutch fettlers thereabouts have given this bird the name of *Honig-wyzer*, or Honey-guide, from its quality of difcovering wild-honey to travellers. Its colour has nothing ftriking or beautiful, as will appear from the defcription and drawing annexed; and its fize is confiderably fmaller than that of our Cuckow in Europe: but in return, the inftinct which prompts it to feek its food in a fingular manner, is truly admirable. Not only the Dutch and Hottentots, but likewife a fpecies of quadruped, which the Dutch name a *Ratel* (a), are frequently conducted to wild bee-hives by this bird, which as it were pilots them to the very fpot. The honey being its favourite food, its own intereft prompts it to be inftrumental in robbing the hive, as

(a) Probably a new fpecies of badger.

fome fcraps are commonly left for its fupport. The morning and evening are its times of feeding, and it is then heard calling in a fhrill tone *cherr*, *cherr*, which the honey-hunters carefully attend to as the fummons to the chace. From time to time they anfwer with a foft whiftle, which the bird hearing, always continues its note. As foon as they are in fight of each other, the bird gradually flutters towards the place where the hive is fituated, continually repeating its former call of *cherr*, *cherr*: nay, if it fhould happen to have gained a confiderable way before the men (who may eafily be hindered in the purfuit by bufhes, rivers, and the like) it returns to them again, and redoubles its note, as if to reproach them with their inactivity. At laft the bird is obferved to hover for a few moments over a certain fpot, and then filently retiring to a neighbouring bufh or other refting-place, the hunters are fure of finding the bees neft in that identical fpot, whether it be in a tree, or in the crevice of a rock, or (as is moft commonly the cafe) in the earth. Whilft the hunters are bufy in taking the honey, the bird is feen looking on attentively to what is going forward, and waiting for its fhare of the fpoil. The bee-hunters never fail to leave a fmall portion for their conductor, but commonly take care not to leave fo much as would fatisfy its hunger. The bird's appetite being only whetted by this parfimony,

it is obliged to commit a fecond treafon, by difcovering another bees-neft, in hopes of a better falary. It is further obferved, that the nearer the bird approaches the hidden hive, the more frequently it repeats its call, and feems more impatient.

I have had frequent opportunities of feeing this bird, and have been witnefs of the deftruction of feveral republicks of bees, by means of its treachery. I had however but two opportunities of fhooting it, which I did to the great indignation of my Hottentots. From thofe fpecimens (both of which are fuppofed to be females) I have made the fubfequent defcription. The inhabitants in general accufe the fame bird of fometimes conducting its followers where wild beafts and venomous ferpents have their places of abode: this however I never had an opportunity of afcertaining myfelf; but am apt to believe fuch cafes to be accidental, when dangerous animals happen to be in the neighbourhood of a bees-neft.

Whilft I ftaid in the interior parts of Africa, a neft was fhewn to me, which fome peafants affured me was the neft of a Honey-guide. It was woven of flender filaments or fibres of bark, in the form of a bottle. The neck and opening hung downwards, and a ftring in an arched

fhape

ſhape was ſuſpended acroſs the opening, faſtened by the two ends, perhaps for the bird to perch upon.

DESCRIPTIO CUCULI INDICATORIS.

ROSTRUM craſſiuſculum, verſus baſin fuſcum, apice luteum.

Angulus oris uſque infra oculos extenſus.

Nares poſtremæ ad baſin roſtri, ſupremæ vicinæ ut carinulâ dorſali ſaltem ſepararentur, oblongæ, margine prominulo.

Pili aliquot ad baſin roſtri, præcipuè in mandibulâ inferiore.

Lingua plana, ſubſagittata.

Oculorum irides ferrugineo-griſeæ.

Palpebræ nudæ, nigræ.

Pedes nigri, ſcanſorii. *Tibiæ* breves; *Ungues* tenues, nigri.

Pileus læte griſeus e pennis brevibus latiuſculis.

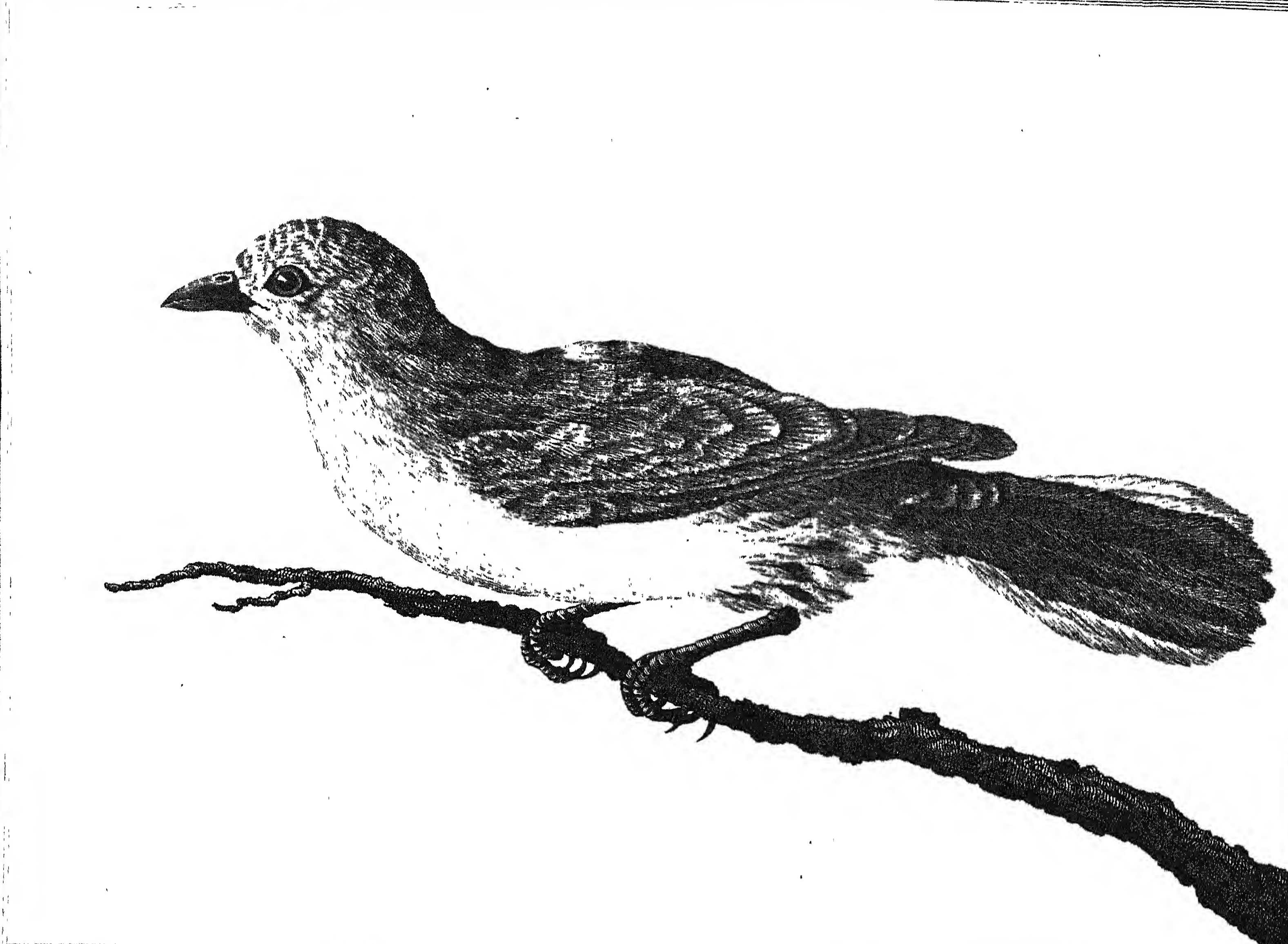
Gula, *Jugulum*, *Pectus*, ſordidè alba, cum aliquo virore vix notabili in pectore.

Dorſum et *Uropygium* ferrugineo-griſea.

Abdomen, *Criſſumque* alba.

Femora tecta pennis albis, macula longitudinali nigra notatis.

4 *Alarum*



CUCULUS INDICATOR. *The Honey Guide.*

Basire sc.

Alarum tectrices ſuperiores omnes griſeo-fuſcæ, exceptis ſummis aliquot quæ flavis apicibus formant *maculam flavam* in humeris, exiguam, et a plumis ſcapularibus ſæpe tectam.

Tectrices infra alam albidæ, harum ſupremæ ex albido nigroque maculatæ.

Remiges omnes ſupra fuſci, primarii octo, ſecundarii ſex, ſubtus cinereo-fuſci.

Alulæ griſeo-fuſcæ.

Cauda cuneiformis, rectricibus duodecim: harum duæ intermediæ longiores anguſtiores, ſupra et infra æruginoſo-fuſcæ; proximæ duæ fuliginoſæ, margine interiore albicantes; duæ utrinque his proximæ, albæ, apice fuſcæ, et exterius ad baſin macula nigra notatæ; extima utrinque reliquis brevior, alba, apice fuſca, macula nigra vix ulla ad baſin.

Alæ complicatæ caudæ partem quartam attingunt.

Longitudo ab apice roſtri ad extremum caudæ circiter ſeptem uncias pedis Anglicani explet.

Roſtrum a baſi ſuperiore ad apicem ſemunciale.

VI. *An Account of ſome new Electrical Experiments. By Mr.* Tiberius Cavallo: *communicated by Mr.* Henley, *F. R. S.*

DESCRIPTION AND USE OF THE ATMOSPHERICAL ELECTROMETER.

Read Dec. 19, 1776.

FIG. I. repreſents a very ſimple inſtrument, which I have contrived for making obſervations on the electricity of the atmoſphere, and which on ſeveral accounts ſeems to be the moſt uſeful inſtrument hitherto invented for that purpoſe. AB is a common jointed fiſhing-rod, without the laſt or ſmalleſt joint. From the extremity of this rod proceeds a ſlender glaſs tube C, covered with ſealing-wax, and having a cork D at its end, from which a pith-ball electrometer is ſuſpended. HGI is a piece of twine faſtened to the other extremity of the rod, and ſupported at G by a ſmall ſtring FG. At the end I of the twine a pin is faſtened, which, when puſhed into the cork D, renders the electrometer E uninſulated.

When

When I intend to obſerve the electricity of the atmoſphere with this inſtrument, I thruſt the pin I into the cork D, and holding the rod by its lower end A, project it out of a window in the upper part of the houſe, into the air, raiſing the end of the rod with the electrometer ſo as to make an angle of about 50° or 60° with the horizon. In this ſituation I keep the inſtrument for a few ſeconds, and then pulling the twine at H, I diſengage the pin from the cork D, which operation cauſes the ſtring to drop in the dotted ſituation L K, and leaves the electrometer inſulated, and electrified with an electricity contrary to that of the atmoſphere. This done, I withdraw the inſtrument, and examine the quality of the electricity without any obſtruction either from wind or darkneſs.

With this inſtrument I have made obſervations on the electricity of the atmoſphere ſeveral times in a day, and have kept a journal of thoſe experiments from the 27th of September laſt to this day.

The following is the moſt remarkable part of the above-mentioned journal, in which I have noted the electricity of the electrometer, that is the contrary of that in the atmoſphere.

The ſtroke —— ſignifies *as above.*

Time of Obſervation.	Clouds.	Fog.	Wind.	Opening of the Electrometer in inches.	Electricity.
Oct. 19th, $10\frac{1}{2}$ o'clock.	Cloudy.	Very little at a diſtance.	Very ſtrong.	$\frac{1}{10}$	Negative.
11	——	——	——	——	——
2	Heavy clouds.	——	Violent.	$\frac{3}{4}$	Poſitive.
$2\frac{1}{4}$	Leſs cloudy.	——	Little.	1	——
3	Few at a diſtance.	——	——	$\frac{1}{2}$	Negative.
8	o	o	——	——	——
Oct. 31ſt, 11 poſt mer.	——	——	o	$\frac{1}{2}$	——
Nov. 6th, 11 poſt mer.		Very thick.	——	1	——

From the above-mentioned journal I have deduced the following general obſervations.

1ſt, That there is in the atmoſphere at all times a quantity of electricity; for whenever I uſe the above deſcribed atmoſpherical electrometer it always acquires ſome electricity.

2dly, That the electricity of the atmoſphere or fogs is always of the ſame kind, namely poſitive; for the electrometer is always negative, except when it is evidently influenced by heavy clouds near the zenith.

3dly, That the ſtrongeſt electricity is obſervable in thick fogs, and the weakeſt when the weather is cloudy and there is a ſtrong appearance of rain; but it does not ſeem to be leſs at night than in the day-time.

DESCRIP-

DESCRIPTION OF THE ELECTROMETER FOR THE RAIN.

THE rain-electrometer is, in its principle, nothing more than an insulated instrument to catch the rain, and by a pith-ball electrometer to show the quantity and quality of its electricity.

Fig. 2. represents an instrument of this kind, which I have frequently used, and after several observations have found to answer very well. ABCI is a strong glass tube about two feet and a half long, having a tin funnel DE cemented to its extremity, which funnel defends part of the tube from the rain. The outside surface of the tube from A to B is covered with sealing-wax, as also that part of it which is covered by the funnel. FD is a piece of cane, round which several brass wires are twisted in different directions, so as to catch the rain easily, and at the same time to make no resistance to the wind. This piece of cane is fixed into the tube, and a slender wire proceeding from the former goes through the bore of the tube, and communicates with the strong wire AG, which is thrust into a piece of cork fastened to the end A of the tube. The end G of the wire AG is formed into a ring, from

which I fufpend a more or lefs fenfible pith-ball electrometer as occafion requires.

This inftrument is faftened to the fide of the window-frame, where it is fupported by ftrong brafs hooks at CB. The part H ftands out of the window, with the end F a little elevated above the horizon. The remaining part of the inftrument paffes, through a hole in a glafs of the fafh, into the room, and no more of it touches the fide of the window than the part BC.

When it rains, efpecially in flying fhowers, this inftrument, ftanding in the fituation above defcribed, is frequently electrified; and by the diverging of the electrometer the quantity and quality of the electricity of the rain may be obferved, without any danger of miftake. With this inftrument I have obferved that the rain is generally electrified negatively, and fometimes fo ftrongly, that I have been able to charge a fmall coated phial at the wire AG.

This rain-electrometer fhould be fixed in fuch a manner that it may be eafily taken from and replaced at the window as occafion may require; for it will be neceffary to clean it very often, particularly when a fhower of rain is approaching.

EXPERIMENTS MADE WITH A GLASS TUBE HERMETICALLY SEALED, AND HAVING SOME QUICKSILVER INCLOSED IN ITS CAVITY.

IN making ſome experiments rather foreign to electricity, it occurred to me, that when I agitated ſome quickſilver in a glaſs tube hermetically ſealed, and in which the air was very much rarefied, it contracted a very ſenſible quantity of electricity; which however was not conſtant, nor, as I firſt thought, in proportion to the agitation of the quickſilver. Being deſirous of aſcertaining the properties of this tube, I conſtructed ſeveral of them, and, as accurately as I could, obſerved their properties; but as they all agreed with regard to the chief points, I ſhall only deſcribe one, which is the beſt I have yet made.

This tube is two feet and ſeven inches long, and about four tenths of an inch in diameter: the quickſilver in it may be about three quarters of an ounce, and to exhauſt it of air, I cloſed it whilſt the quickſilver was boiling in its oppoſite end.

Before I uſe this inſtrument I warm it a little and clean it; then holding it nearly horizontally, I let the quickſilver in it run from one end of the tube to the other,

other, by gently and alternately elevating and depreſſing its extremities. This operation immediately renders the outſide of the tube electrical, but with the following remarkable property, *viz.* that part or end of the tube where the quickſilver actually ſtands is poſitive, and the remaining part negative. If, by elevating this poſitive end of the tube a little, I let the quickſilver run to the oppoſite end which was negative, then the former inſtantly becomes negative and the latter poſitive. The poſitive end is always more ſtrongly electrical than the negative. If when one end of the tube (which we call A) is poſitive, that is, if, when the quickſilver is in it, I do not take off the electricity; then on elevating it ſo as to let the quickſilver run to the oppoſite end B, the end A becomes negatively electrified in a very ſmall degree: if I make it poſitive a ſecond time, and do not take off that poſitive electricity; then, on elevating it again, it appears to be poſitive in a ſmall degree: but if whilſt it is poſitive I take off that poſitive electricity, then on being elevated it appears ſtrongly negative.

Theſe appearances I would explain in the following manner: the quickſilver agitated within the cavity of the tube acts like a rubber, that is, excites the inſide ſurface of the tube poſitively, and becomes itſelf negative. Now when the quickſilver, negatively electrified, is on one

end of the tube, the outſide of the glaſs, by the known property of charged electrics, muſt be poſitive. The remaining part of the tube being poſitive on its inner ſurface, muſt be negative on the outſide. But as there is a *vacuum* within the tube, it may be aſked, why is not the equilibrium between the negative electricity of the quickſilver and the poſitive electricity of the glaſs inſtantly balanced?

When about two inches of each extremity of this tube are coated with tin-foil, that coating aſſiſts to make the electricities more conſpicuous.

With regard to the conſtruction of ſuch tubes (which I have made of ſeveral lengths from nine inches to two feet ſeven inches) I find that ſome will act very well, while others will hardly acquire any electricity at all, even when they are made very hot. I am not as yet thoroughly ſatisfied with reſpect to the cauſe of this difference, but ſuſpect that the thickneſs of the glaſs is more concerned in it than any thing elſe; for I find that a tube whoſe glaſs is about one twentieth of an inch thick, anſwers better than either a thicker or a thinner one.

November 13, 1776.

VII. *A Third Essay on* Sea-anemonies. *By the Abbé* Dicquemare.

ON THE GENERATION OF THE FOURTH SPECIES.

Read Jan. 9, 1777.

HAVING in my second essay on sea-anemonies, communicated my discoveries on the manner in which the fourth species is multiplied, and offered some conjectures on the internal organization of the small shreds which become anemonies, I added a caution, not to admit of those conjectures without circumspection. What passed under my own eyes convinced

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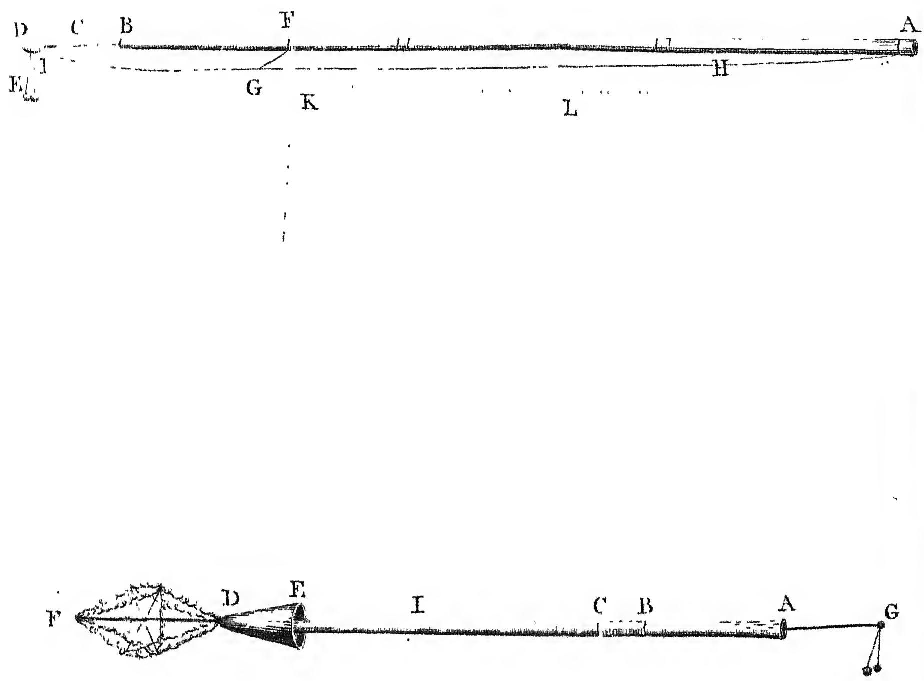
Troisieme Mémoire pour servir à l'Histoire des Anémones de Mer. *Par M. l'Abbé* Dicquemare.

Sur la Génération de la Quatrieme Espece.

Havre de Grace, Mars 7, 1776.

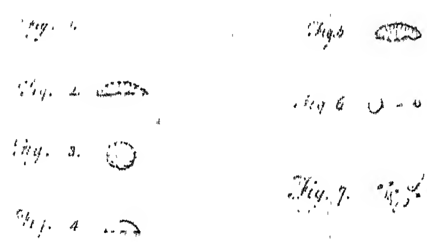
APrès avoir exposé dans un second mémoire pour servir à l'histoire des Anémones de mer, mes découvertes sur la maniere dont celles de la quatrieme espece se multiplient; après avoir ouvert quelques conjectures sur l'organisation intérieure des petits lambeaux qui deviennent anémones, je ne dissimulai pas qu'on ne devoit point y restraindre ses idées. Ce qui se passoit sous mes yeux me

Fig. 1



Cavallo delin.

Tab.III. p.56.



Basire sc.

convinced me more and more (and I gave ſome hints to that purpoſe) that the ſmalleſt particle of a living animal, has an organization which far exceeds every idea we can conceive of it; and which, from the extreme minuteneſs of thoſe particles, baffles our cloſeſt inſpection: ſo that inſtead of being ſurprized at the ſingular effects of reproduction, they are rather what we ought to expect, and be prepared to obſerve as they ariſe. It is with this view that I have continued my experiments and obſervations: they have confirmed the diſcoveries I had made, and afforded me an opportunity to juſtify ſome eminent men, whoſe aſſertions concerning the multiplication of the freſh-water polypi by ſections, have met with the moſt unmerited contradictions.

Among all the objects which nature offers to the contemplative mind, there is none ſo ſtriking and important as that of the generation of beings, and eſpecially of animated

me perſuadoit de plus en plus, et je m'en expliquai, que les moindres parties d'un être vivant ont une organiſation qui ſurpaſſe infiniment l'idée que nous pouvons nous en faire, que l'enorme petiteſſe de ces parties dérobe aux regards les plus avides; et que loin de ſe ſurprendre des éffets ſinguliers de reproduction, on devroit pour ainſi dire les attendre, et ſe mettre à portée de les ſaiſir. C'eſt dans cette vüe que j'ai continué mes obſervations et mes expériences: elles ont confirmé les découvertes que j'avois faites, et me procurent la ſatisfaction de juſtifier les hommes illuſtres, qui en nous faiſant connoitre la multiplication des polypes d'eau douce par la ſection, ont eprouvé les contradictions les moins méritées.

De tous les objets que la nature offre à l'eſprit méditatif, il n'en eſt point de plus grand, de plus reſpectable, que celui de la génération des êtres, et ſurtout des êtres

mated beings. This grand myſtery has always attracted the attention of the greateſt philoſophers; but their want of ſucceſs in their reſearches may be eaſily inferred from the deficiency of proper means of obſervation: and it ſeems an advantage reſerved for our age, to introduce a new ſet of beings, which bids fair to throw great light upon our enquiries on that ſubject.

Had I only freſh proofs to add to thoſe already given, and no new diſcoveries to commmunicate, I ſhould certainly leave nature to eſtabliſh our opinion of her operations: but having been fortunate enough to gather ſome ſcattered fruits in this wide field, which had eſcaped the obſervation of others merely becauſe they were engaged in more plentiful harveſts, I have been ſtruck with the increaſing ſingularity, number, and importance of the objects, which have appeared to me to diſſipate all doubt. I ſhall

êtres vivans: c'eſt auſſi vers cette merveille que les philoſophes les plus diſtingués ont dirigé leurs regards; mais leur manque de succés reſulte naturellement des foibles moyens qu'ils avoient pour faire des obſervations; et c'eſt un avantage qui paroit avoir été reſervé à notre ſiecle, de faire paroître ſur la ſcene des êtres juſqu'ici ignorés, qui promettent de nous fournir des nouvelles lumieres ſur ce ſujet.

Si je n'avois à préſenter ici que les mêmes découvertes, quoique ce fût multiplier les preuves, je laiſſerois à la nature le ſoin de fixer l'opinion; mais dans ce champ fertile j'ai été aſſez heureux pour recueillir quelques fruits à l'écart, et qui n'avoient échappé aux regards des autres que parcequ'ils etoient occupés de recoltes plus abondantes: j'ai vû la ſingularité s'augmenter, et le nombre comme l'etendüe des objets m'a paru propre à diſſiper les doutes. Je ne rappellerai pas ici ce

I ſhall not here repeat what I have already ſaid concerning the reproduction of the ſuperior part of the fourth ſpecies, which is often as thick as one's arm; new experiments have confirmed it, and have ſhewn the poſſibility of the reproduction of freſh-water polypi, without having recourſe to a multitude of imperceptible animals: but the better to explain what I have ſince diſcovered concerning the generation of this ſpecies of anemonies, I muſt beg leave briefly to recapitulate what I have already publiſhed on that ſubject. Theſe anemonies, having their baſes unequally extended upon, and firmly adhering at their extremities to a hard ſubſtance, contract; and thus tear off and leave on that hard ſubſtance, one or more ſmall ſhreds of their baſes, covered with pieces of the coat of the old animals; and theſe ſhreds ſoon after become ſmall anemonies, which alſo is the caſe in artificial ſections. Of this ſingular operation I have had opportunities

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ce que j'ai dit de la reproduction de la partie ſupérieure des anémones de la quatrieme eſpece, ſouvent groſſe comme le bras; de nouvelles expériences l'ont encore confirmée, et ont fait voir la poſſibilité de celle des polypes d'eau douce, ſans avoir beſoin de recourir à une multitude d'animaux imperceptibles: mais qu'il me ſoit permis de retracer ici en peu de mots ce que j'ai déja publié ſur la génération de cette eſpece, autrement il ſeroit difficile de ſaiſir ce que j'ai apperçu depuis. Ces anémones, ayant la baſe inégalement étendüe et fortement attachée par quelques points de ſes extremités ſur un corps dur, ſe retirent ſur elles mêmes, et laiſſent ainſi en ſe déchirant une ou pluſieurs portions très petites de leur baſe, recouvertes d'une parcelle de leur robe, qui deviennent en peu de temps des petites anémones, ce qui a lieu auſſi par des ſections violentes. En obſervant de nouveau ces animaux

nities of feeing repeated inftances, having been very affiduous and conftant in my obfervations.

On the 26th of October 1775, an anemony, on which I had tried an experiment foreign to the prefent purpofe, contracted, and left on the fide of the vafe a fmall fhred, which at the very firft I fufpected to be intended for a young anemony: not that it was either flefhy, or that it feemed to contain a bulb; but becaufe the anemony had for feveral days before ftretched itfelf confiderably, and in a particular manner, from that point of the bafe. Certain internal fibres or *radii* appeared; which in the old anemony had their direction from the circumference to the centre: and as the fhred was an irregular fegment of the area of the circle formed by the bafe of the old anemony, it is evident that thefe fibres or *radii*, being fomewhat diftant from each other at the arch of the fegment, did

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animaux, et après avoir attendu longtemps, j'ai eû la fatisfaction de voir fe répéter cette opération finguliere, et je l'ai fuivie d'auffi près qu'il m'a été poffible.

Le 26 Octobre, 1775, une anémone fur laquelle j'avois tenté une expérience qui n'a rien de commun avec notre objet prefent, laiffa contre les parois du vafe, en fe retirant, un petit lambeau que je foupçonnai dés le commencement être deftiné à devenir une petite anémone; non qu'il fut épais ou qu'il me parut contenir quelque bulbe, mais parceque l'anémone s'etoit fort alongée par ce point de fa bafe, depuis plufieurs jours, d'une maniere toute particuliere. On apperçevoit dans l'intérieur certains fibres ou rayons qui dans l'anémone avoient tendu de la circonférence au centre, et comme ce lambeau etoit un fegment irrégulier de l'aire du cercle que forme la bafe d'une grande anémone, on comprend que ces fibres ou rayons, un peu diftans l'un de l'autre à l'arc de ce fegment, ne convergoient pas

did not converge ſufficiently to form a centre at the chord; and that the point of union of theſe *radii* was the centre of a circle equal to that formed by the baſe of the old anemony (ſee TAB. III. fig. 1. its natural ſize, and fig. 2. magnified). During the firſt days this little fragment acquired plumpneſs, bent itſelf gradually into a round figure, and ſeemed to make ſome efforts towards forming itſelf into the ſhape in which it appeared on the 25th; the fibres becoming gradually more convergent, the chord of the arch ſhorter, and the arch a ſegment of a ſmaller circle. At length the *radii* united in a centre (ſee fig. 3. magnified) and its profile appeared a ſegment of a ſphere (ſee fig. 4.)

On the 30th of October I perceived in this fragment conſiderable contractions and dilatations in the direction of its thickneſs; but could ſee no mouth nor limbs. On the 1ſt of

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pas aſſéz pour former un centre à la corde; et que le point de réunion de ces rayons etoit le centre d'un cercle égal à celui que formoit la baſe de l'anémone: voyéz TAB. III. fig. 1. et 2. dans l'une il eſt de grandeur naturelle, et dans l'autre vû à la loupe. Pendant les premiers jours cette petite portion prenoit de l'epaiſſeur, ſe recourboit et s'arrondiſſoit peu à peu; elle tendoit de toute ſes forces à prendre la forme ou elle parvint le 25, c'eſt à dire que petit à petit, les fibres etant devenus plus convergens, la corde de l'arc du cercle plus courte, l'arc une portion d'un plus petit cercle, il s'etoit formé un centre de réunion de ces rayons, qui occupoit le côté de ce petit corps animé, comme le repreſente la figure 3. qui eſt vüe à la loupe; quant au profil, il eſt répreſenté par un ſegment de ſphere, ou par la figure 4.

Le 30 Octobre j'apperçus des contractions et des dilatations très ſenſibles dans l'épaiſſeur, mais point de bouche ni de membres. Le 1 Novembre il a changé de

 place.

of November it moved from its place. On the 7th I diſcovered, by means of a ſtrong lens, an orifice and ſome appearance of limbs. On the 16th I ſaw them very diſtinctly. On the 17th it changed its place again. At the beginning of January 1776, the folds of the body were formed, and then it could not but be conſidered as a young anemony, ſimilar in every reſpect to the old one by which it had been produced, except in the number of limbs, which however, although the animal be ſtill very ſmall, are now increaſing. The ſemi-tranſparency, which often prevents accurate obſervation, allowed me to view the gradation through which the ſmall fibres became convergent: I could alſo ſee the gradual contraction of the angles of the ſegment, and that not a ſingle particle of the fragment periſhed; all ſeemed to be re-incorporated into the maſs. The word *all* muſt not, however, be taken in a ſtrict ſenſe; for I muſt confeſs that I perceived ſome yellowiſh filmy matter round this little

maſs,

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place. Le 7, à l'aide d'une forte loupe, j'ai apperçu un orifice, et des apparences de membres. Le 16 je les ai vûs très diſtinctement. Le 17 il a de nouveau changé de lieu. Au commencement de Janvier 1776, le pli du corps s'eſt formé; c'etoit donc alors une petite anémone qui, à l'exception du nombre des membres, reſſembloit parfaitement à celle dont elle etoit provenüe; les membres augmentent, et cependant cette anémone eſt fort petite. La demi-tranſparence, qui gêne quelquefois dans les obſervations, m'a permis de diſtinguer le progrès par lequel les petites fibres ſont devenües convergentes: J'ai vû auſſi les angles de ſegment ſe racourcir, et rien ne m'a paru périr: tout s'eſt refoulé dans la maſſe. Ce mot *tout* ne doit cependant pas être pris à la lettre: j'ai apperçu quelques pellicules quelques,

ſubſtances

maſs, which ſeparated from it; but its quantity was ſo ſmall, that I could not thence infer the loſs of any particle of the ſhred, and rather imagined that this matter was merely the effect of ſome ſecretions or extravaſated humour.

What ſtill leaves me a doubt concerning the germ is, that this effect has been more conſiderable in ſome of my experiments than in others. Had the fragment of the anemony contained a germ, it might be concluded that the membrane which covered it ſhould have periſhed as ſoon as the animal was formed; this however was not ſufficiently evident in ſeveral caſes, and eſpecially in thoſe where the fragment had been naturally torn off. At firſt the ſhred was thin, and thinner ſtill before it was ſeparated from the old anemony; and no bulb could ever be perceived either then or in the ſequel. I was then led to imagine,

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ſubſtances un peu jaunâtres, autour de la petite maſſe, qui en ont été ſeparées; mais elles etoient en ſi petite quantité, que je n'ai pu en conclure qu'il eut péri quelque choſe du lambeau, cette ſubſtance etoit peut-êtré düe à des ſécrétions ou à quelque humeur extravaſée.

Ce qui me laiſſe encore un doute réel, c'eſt que cet effet a été plus ſenſible dans quelques unes de mes expériences. Si cette portion d'anémone eût contenu un germe, n'eſt-on pas porté à penſer que les membranes qui l'envelopent auroient péri lors de ſon dévelopement. C'eſt ce qu'on ne remarque pas d'une maniere aſſéz ſenſible dans pluſièurs, et ſurtout dans ceux qui ſe déchirent naturellement. D'abord le petit lambeau étoit mince et, avant qu'il fût ſéparé de l'anémone, je l'ai obſervé pendant pluſieurs jours; il etoit plus mince encore: je n'y voyois nulle apparence

imagine, especially on account of the union of the fibres in a common centre, that a simple shred produced an anemony. But from what cause does this little fragment contract into a circle, and increase its thickness? how shall we account for its visible tendency towards forming a new animal? is it a necessary instinct? can it be admitted that in these animals the vital principle is peculiar to every particle? and in what manner is it, or is it not, subordinate to the general organization of the individual, according to circumstances which it seems in our power to modify? How many specious reasonings might be made on this subject, did we prefer the admiration of the public to real truth! The only inference that I think can as yet be derived from these and the following observations is, that there certainly are animated beings which multiply, as it were, by slips; but whether the concurrence

apparence de bulbe, et je n'y en ai point vu depuis: j'ai donc été tenté de croire, surtout à cause de la réunion du bout des fibres à un centre commun, que c'etoit un simple lambeau qui devenoit anémone. Mais qui est ce qui fait, qu'étant détachée, cette petite portion se recourbe, et prend de l'épaisseur? par quelle cause tend-elle visiblement à former un animal?. sent-elle alors la necessité de le devenir? le principe de la vie feroit-il, dans ces animaux, particulier à chacune de leurs parties; et comment est il, ou n'est il pas, selon les circonstances que nous pouvons faire naitre, subordonné à l'organisation générale de l'individu? Oh, si on étoit tenté de préferer l'admiration du public à la bonne foi, que de choses à dire! Tout ce qui me semble qu'on peut conclure de ces observations, et plus encore de celles qui vont suivre, c'est qu'il est vrayment des êtres animés qui se multiplient comme de bouture; mais j'attends être instruit par des nouvelles

currence of two ſexes may or may not be diſpenſed with, is a fact I expect to learn from future experiments. I can at preſent only ſuſpect its being unneceſſary, ſince the anemonies I had hitherto operated upon were all produced at ſea. The manner in which they multiply ſeems ſtrongly to favour that ſuppoſition: ſuch ideas begin already to become familiar to us, they appear leſs ſingular; and yet how different are they from thoſe we uſed to entertain of the animal creation!

Let us now proceed from the operations of mere nature, to ſuch as are in ſome meaſure the effect of art. On the 12th of December 1775, I ſevered with the point of a knife ten ſmall pieces from the baſes of ſeveral anemonies, at the places where they ſeemed the moſt diſtended, and moſt adherent to large oyſter-ſhells, from which they dropped after the operation. I put theſe pieces

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nouvelles expériences, ſur l'inutilité du concours des deux ſexes: je ne puis que la ſoupçonner, parceque les anémones que j'ai opéré juſqu'ici etoient nées en mer. La maniere dont ces animaux ſe multiplient ſemble étayer fortement ce ſoupçon. Déja nous nous accoutumous à toutes ces idées nouvelles; elles nous frapent moins: cependant combien ſont elles éloignées de celles que nous avions de l'animal!

Paſſons maintenant des opérations propres de la nature, à celles où l'art entre pour quelque choſe. Le 12 Décembre 1775, je coupai, avec le bout d'un biſtouri, dix petites portions des bords de la baſe de pluſieurs anémones, aux endroits ou ces baſes etoient plus etendües, et plus adhérentes à des écailles de groſſes huitres, dont elles ſe détachoient en les coupant. Je mis ces particules dans

pieces into different vafes. The next day two of them ftuck to the bottom; on the 14th two more adhered; on the 22d, fix of them; on the 24th, nine; and on the 27th, all were affixed. Each of them went through the fame progrefs as the pieces which had been torn off naturally; and before the beginning of March they were all furnifhed with limbs &c. Although I be confcious of having been very accurate and attentive in my obfervations, ftill I have not yet been fortunate enough to fee all that I could wifh. I fhall however repeat my attempts, and I have great expectations from the new experiments I meditate. The prefent have pointed out fome differences which lead to feveral reflections. Thofe fhreds which had been cut of a larger fize, produced (befides the films they may have loft) larger anemonies. Whether this multiplication be the effect of a bulb, of a germ, or

dans autant de vafes: le lendemain deux etoient attachées au fond; le 14 deux autres etoient de même attachées; le 22 fix, le 24 neuf, et le 27 elles l'etoient toutes. Chacune d'elles m'ayant préfenté fucceffivement les mêmes progrès qu'avoient fait les portions détachées naturellement, il féroit inutile de les décrire de nouveau: avant le premier Mars elles avoient toutes des membres, &c. Quoique j'aye obfervé avec le plus grand foin ce qui s'eft paffé, je n'ai pas encore été affez heureux pour voir tout ce que je défirois; mais j'y reviendrai et j'efpére beaucoup des nouvelles tentatives que je médite. Celles-ci m'ont offert des différences qui donnent lieu à quelques réflexions. Ceux de ces petits morceaux d'anémones que j'ai coupés plus grands, ont produit (indépendamment de ce qui a pu s'en détacher) des anémones plus grandes. Si c'eft en vertu d'une bulbe, d'un germe,

or of an egg, it ſtill appears that its teguments, and all that is contiguous to thoſe teguments, ſhould not be a conſtituent part of the animal, and that only a larger germ can produce a larger animal.

Is it then in our option to produce anemonies not only when we pleaſe, but alſo of what ſize we pleaſe[(a)]? Or does the multiplying anemony follow in this its own inclination? All this, added to the junction of the fibres, ſeems very oppoſite to the opinion of the germs or eggs; but on the other hand there are obſervations which fa-

(a) Without invalidating what is here ſaid, ſome experiments have induced me to think that this aſſertion ſhould be admitted with ſome limitations; that if the ſhreds be very large, they will periſh; that in general only ſmall ones ſhould be cut, without either fretting or tearing them; and that the vaſes ſhould always be kept very clean, and the water as clear and as freſh as poſſible.

vour

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germe, ou d'un oeuf quelconque, que cette multiplication ſe fait, il ſemble que tout ce qui y ſeroit joint comme envelope, et plus encore comme contigu aux envelopes, devroit ne pas faire partie du petit animal, et qu'il n'y auroit qu'un germe plus gros, qui donnât une anémone plus groſſe.

Sommes nous donc les maitres, non ſeulement de faire naitre les anémones quand nous voulons, mais même de leur donner plus ou moins de groſſeur (a)? L'anémone qui multiplie en diſpoſe-t-elle auſſi à ſon gré? Tout cela joint à la réunion des fibres, ſemble bien oppoſé aux germes et aux œufs; mais auſſi d'autres obſervations leur ſont favorables. Il m'a ſemblé qu'il ſe ſéparoit un peu plus de

(a) Sans affoiblir ce que j'éxpoſe, quelques expériences me font penſer que ceci a des bornes aſſéz étroites; que des morceaux trop grands périſſent, et qu'en général il n'en faut couper que de petits, ſans les tirailler; que les vaſes doivent être nets, et l'eau claire et fraiche.

ces

vour it. It appeared to me, that more of the membranaceous particles came off from the ſhreds which had been clipped of a larger ſize, than from thoſe which had been naturally torn from the anemony; but as I have not had occaſion to obſerve many of the latter, this difference may be owing merely to the different ſizes of the ſhreds.

Another circumſtance I obſerved in the pieces that were naturally torn off is, that there are ſome which produce ſeveral anemonies, which ſometimes remain united, and at other times ſeparate. I have myſelf frequently ſeen this operation. One of the ſhreds I had clipped was of an irregular ſhape, nearly as in fig. 5. A little contraction was ſoon formed between the two extremities, both of which became round, ſwelled and aſſumed the appearance of two drops of tallow; the contraction became like

ces eſpeces de pellicules des morceaux coupés un peu gros, que de ceux arrachés naturellement par l'anémone même; mais comme je n'ai eu occaſion d'obſerver qu'un petit nombre de ceux-ci, il pourroit arriver que cette différence ne feroit düe qu'au plus ou moins d'étendüe du lambeau.

Une autre remarque que j'avois déja faite dans les morceaux ſéparés naturellement eſt, qu'il s'en trouve quelques uns d'où naiſſent pluſieurs anémones, entre les quelles il y en a qui reſtent unies, tandis que d'autres ſe ſéparent. Cette opération s'eſt répêtée ſous mes yeux. Un lambeau que j'avois coupé avoit à peu près la forme que repreſente la figure 5. Il ſe forma un petit étranglement entre l'un et l'autre de ſes bouts; chaque bout prit une figure ronde, et leur ſurface ſuperieure s'elevoit en goute de ſuif: l'étranglement devint comme un filet,

like a thread (fig. 6.). On the 24th of January, the largeſt piece (for they were of very unequal ſizes) crept up a little way on the ſide of the vaſe; and on the 28th, the thread broke, when the two fragments became two diſtinct anemonies. Doth this imply that there were two germs in the ſhred? Or may one ſingle ſhred, without any bulb, germ, or egg whatever, produce one or more anemonies either connected or ſeparate? Theſe are queſtions I am not yet able to ſolve, and I wiſh I were the only one thus uninformed. But to return; the following facts, of which I have frequently been eye-witneſs, muſt now be admitted. 1ſt, That the anemonies of this ſpecies are multiplied by ſhreds, both naturally and artificially. 2dly, That theſe ſhreds produce ſometimes only one, and at other times ſeveral anemonies. 3dly, That among theſe young anemonies formed by one ſhred, ſeveral remain

filet, figure 6. Le 24 Janvier, la plus groſſe partie (car elles etoient fort inégales) monta un peu aux parois du vaſe; le 28 le filet s'eſt rompu, et ces deux parties ſont devenües deux petites anémones. Y auroit il donc eu deux germes dans ce morceau? ou bien peut-il d'un ſimple lambeau ſans bulbe, ſans germe, ſans œuf quelconque, naitre une ou pluſieurs anémones, ünies ou ſéparées? C'eſt ce que je ne ſçais pas encore, et je voudrois être le ſeul qui l'ignorat. Revenons donc à cette ſource féconde. D'abord on doit regarder comme des faits certains, dont j'ai été temoin pluſieurs fois, 1°, que les anémones de cette eſpece ſe multiplient naturellement et artificiellement par des lambeaux; 2°, que ces lambeaux deviennent ſouvent üne, quelquefois pluſieurs anémones; 3°, que de ces petites anémones, formées d'un même lambeau, pluſieurs reſtent unies entre elles quoique

main connected, but that the greatest number separate by contractions. 4thly, That among those which remain connected there are some which grow to the largest size, such as the monstrous anemony mentioned in my second essay, in which three individuals had been blended together; and another of a smaller size in the shape of a Y [(b)], represented in the plate of the same essay, which before my own eyes produced a young anemony, by tearing a small shred from the base of its coat.

What do we then perceive in these shreds? Nothing hitherto but a membrane which was before part of the base of the great anemony, a mere skin which was part of its

(*b*) This species, which is so fertile in monstruosities, has also presented me with one which had two bodies on one base. Of the first species I have as yet seen but one monster, which on the contrary had two bases and only one body. I saw it at its birth.

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quoique le plus grand nombre se sépare par étranglement; 4°, que parmi celles qui restent unies, il y en a qui deviennent de la plus belle grosseur, comme l'anémone monstrueuse dout j'ai parlé dans mon second mémoire, dans laquelle trois individus etoient confondus, et une autre moins grosse en forme d'Y (*b*), representée dans la planche du même mémoire, qui produisit devant moi une petite anémone en déchirant un lambeau du bord de sa base.

Qu'apperçoit-on donc dans l'un de ces lambeaux? Jusqu'ici je n'y ai vû qu'une membrane qui faisoit auparavant partie de la base de la grande anémone, une peau,

(*b*) Cette espece féconde en monstruosité m'en a encore offert une qui a deux corps sur une même base. Je n'ai jamais eu qu'un monstre de la prémiere espece, il avoit au contraire deux basés et un seul corps. Je l'avois vû naitre.

its coat; ſome muſcular fibres and ſmall internal filaments deſcribed in my ſecond eſſay, as they appeared in the ſolar microſcope; and a clammy ſubſtance filling up the interſtices. When ſuch a ſhred is decompoſed, it changes into a whitiſh glutinous ſubſtance, which, through a microſcope, appears a maſs of minute globular bodies, that ſeem ſtill to be of a compound texture, and ſome of which are of a larger ſize, and of an oblong oval ſhape (fig. 7.) as they may be often obſerved in ſea-water viewed through a microſcope. The circular edge of theſe ſhreds which formed part of that of the old anemony, retains the faculty of adhering and looſening itſelf; nor do any of the fibres periſh in the formation of the new animal; they only receive a new arrangement, or acquire a greater convergency. What cauſe can

peau portion de ſa robe, des fibres muſculaires, des petits cordons dans l'intérieur dont j'ai parlé dans mon ſecond mémoire aprés les avoir examinés au microſcope ſolaire, et enfin une matiere gélatineuſe qui remplit les intervalles. Lorſque ces lambeaux ſe décompoſent, il en reſulte une matiere blanchâtre, legérement viſqueuſe, laquelle, vûe au microſcope, offre des eſpeces de corps globuleux, très petits, qui paroiſſent compoſés, et parmi leſquels on remarque une grande quantité d'animaux plus grands, d'un ovale fort allongé, comme le repreſente la figure 7, et comme on en voit ſouvent dans l'eau de la mer éxaminée au microſcope. Le bord en portion de cercle de ces lambeaux, qui formoit celui de la grande anémone, conſerve la faculté de s'attacher et de ſe détacher; et dans la formation du petit animal les fibres ne ſe détruiſent point, ils ne font que prendre comme l'on voit un nouvel arrangement, ou plus de convergence. Quelle peut donc être la

can we then assign to the tendency these fibres and these shreds have to form a new animal? It can easily be conceived that the exterior border of the shred, preserving the faculty of adhering, may form a segment of a smaller circle; but it might equally incline to form a segment of a larger one, did it not seem more natural that a body endowed with sensation, should rather endeavour to close a wound, than to open and rend it more and more. There must then be in this shred a certain degree of sensation, since in order to fix, and to loosen itself, occasionally, it must have a perception of its adherent or detached state. But there is even more than this in the shred we are examining; for allowing that the border assumes a circular shape, and thereby causes a convergency of the fibres, that the angles contract, and the wound closes, the result of which is a small animated body; yet it will always be difficult

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la cause par laquelle ces fibres, et le lambeau entier, tendent à former, et semblent former en effet une petite anémone? On conçoit aisément que le bord extérieur du lambeau, conservant la faculté de s'attacher, peut former une portion d'un plus petit cercle; mais aussi il pourroit en former une d'un grand, si ce n'est qu'il paroit plus naturel pour un corps doüé de la faculté de sentir, de tendre à refermer une playe, que de l'ouvrir, ou de la tirailler de plus en plus. Il y a donc dans ce petit lambeau une sorte de sensation, puisque pour s'attacher et se détacher au besoin, il semble qu'on doit sentir qu'on s'attache ou qu'on se détache. Il y a plus que cela dans ce lambeau que nous observons, car quand le bord s'arrondiroit, que par là les fibres prendroient de la convergence, que les angles se rapprocheroient, que la playe se consolideroit, qu'en résulteroit-il? un petit corps animé?

difficult to conceive how this ſmall ſhred preſerves all theſe faculties, how this animated being acquires the power of loco-motion, and whence proceeds the whole reproduction of an animal. Where ſhall we find the principles of the limbs, of the inteſtines, of the mouth, of the fold in the body, of the elegant tufts which terminate the limbs, &c. ſince nothing of all this can be obſerved in ſuch a fragment? Shall we ſeek for that principle in the ſlender threads, the conſtruction of which, ſeen with the ſolar microſcope, had ſtruck me with admiration? but my obſervations have not convinced me that they were intended for theſe purpoſes. What would have determined my opinion in favour of theſe filaments is, the facility with which they wrap themſelves up in a ſpiral, and form certain parcels (which have the appearance

animé? mais outre qu'il ſera toujours difficile de concevoir comment ce petit lambeau conſerve ces facultés, et comment ce corps animé acquiert celle de changer de lieu au beſoin, on peut encore demander d'où procédera le dévelopement qui ſuit? où ſe trouvera le principe des membres, des inteſtins, de la bouche, du pli du corps? d'où naiſſent ces beaux panaches qui terminent les membres, &c. puiſque rien de tout cela ne ſe remarque dans cette partie? Chercherons nous ce principe dans les cordons déliés dont la ſtructure, au microſcope ſolaire, nous a paru ſi admirable? mais je n'ai pu trouver dans l'obſervation de quoi me convaincre qu'ils ſoient deſtinés à cet uſage. Ce qui eût été bien plus capable de me fixer ſur ces cordons, c'eſt la facilité qu'ils ont de ſe rouler en ſpirale, et de former des eſpeces de paquets qu'on apperçoit comme un petit

pearance of ſmall whitiſh bodies) near the border, between the ſkin and the baſe of the young anemonies, when they extend the latter. I might have been particularly influenced by the number of floating tranſparent globules, which, by means of the microſcope, I have often ſeen among thoſe filaments, and which appeared nearly of the ſame texture: but it muſt be here obſerved that the illuſions of the microſcope are often very great, on account of the ſpherical form and tranſparency of thoſe globules, which at firſt ſight appear to have a large hole diametrically through them. The globules which, by means of the microſcope, are often ſeen in ſea-water, evidently contain a liquid; and here all my knowledge ends. Does the principle we are in ſearch of exiſt in this gelatinous ſubſtance, concerning whoſe texture we are

petit corps blanchâtre vers les bords, entre la peau et la baſe des jeunes anémones, lors qu'elles l'étendent. J'aurois pu encore être arrêté d'une maniere plus particuliere à l'aſpect de certains globules flotans et tranſparens, que j'ai ſouvent trouvés au microſcope parmi ces cordons, et qui me paroiſſent compoſés à peu prés de même, autant que les illuſions microſcopiques permettent d'en juger; car ces illuſions ſont fort grandes à cauſe de la ſphericité et de la tranſparence de ces globules, qui au premier coup d'œuil ſemblent percés diamétralement d'un gros trou; et le microſcope ſolaire ne m'a pas mieux ſervi que les autres à ce ſujet. Ces globules, qui d'ailleurs ſe rencontrent ſouvent dans l'eau de la mer vüe au microſcope, ſont ſans doute remplis d'une liqueur; et voila où ſe terminent toutes mes connoiſſances. Sera-ce dans la ſubſtance gélatineuſe, dont la texture ne nous eſt

are as yet perfectly in the dark? If ever we difcover this texture, it will probably lead us back to a more diftant mechanifm, and this to another of a ftill more delicate order: this laft perceptible caufe, will probably conduct us to the general order, that is to nature; and nature to its Author. But how fatisfactory, how ufeful may it not prove to unravel by degrees (although perhaps with fome uncertainty) even the coarfeft mechanifm by which nature operates? Shall we fuppofe that the gelatinous matter is nothing but an irregular, incoherent fubftance? At firft fight the fame might be faid of the white fubftance of the brain, although it feem to have more confiftency; yet in many places it appears fibrous, and if we could trace it through the nerves, we fhould no doubt difcover a moft admirable organization. The operations of nature in eggs, chryfalids,

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eft point connüe, qu'exiftera le principe que nous cherchons? Si nous la connoiffons un jour, elle pourra bien nous renvoyer à un méchanifme plus eloigné; celui-ci à un d'un ordre plus délicat; et enfin la derniere caufe apperçue, à l'ordre général, c'eft à dire à la nature; et la nature à fon auteur. Mais combien n'eft il pas fatisfaifant, combien ne peut il pas être utile, de déveloper peu à peu, quoiqu'avec quelque incertitude, le méchanifme même le plus groffier par lequel la nature opére? Cette matiere gélatineufe ne feroit elle qu'un corps informe fans texture? à la premiere vûe nous en dirions bien autant, de la fubftance blanche du cerveau, quoique plus compacte; cependant en plufieurs endroits elle paroit fibreufe, et fi nous pouvions y fuivre les nerfs, nous découvririons fans doute une organifation admirable. Ce qui fe paffe dans

lids, nymphs, and a great number of marine bodies, ſeem to juſtify the opinion that there is, in certain humours, a kind of organization which is imperceptible to our eyes, and conceals from us great and important myſteries. All this muſt then be ſuppoſed to exiſt in the gelatinous matter contained in our ſmall fragment: and it is this, perhaps better than the reproduction of parts, that will point out to us at large, the imperceptible operations of nature in the formation of the *fœtus*, of eggs, and perhaps of all that may be called a germ. It is thus that the different views taken of nature may mutually clear up each other. I have not the vain preſumption to think that I ſhall be able to make a great progreſs in this dark career; all I boaſt is, that I have dared to enter into it. I have great expectations from the experiments I have

dans les œufs, dans les chryſalides, dans les nimphes et dans une aſſéz grande quantité de ſubſtances qu'on trouve dans la mer, &c. porte à croire qu'il y a dans certaines humeurs une ſorte d'organiſation qui nous échappe et qui couvre de grandes merveilles. Il faudra donc ſuppoſer tout cela dans la matiere gélatineuſe de notre petit lambeau: là, plus encore que dans les reproductions de parties, elle pourroit nous offrir en grand, ce qui précede en très petit la formation du fœtus dans l'œuf, et peut-être celle de tout ce qu'on peut appeller germe. C'eſt ainſi que les différens points de vüe ſous leſquels on peut conſiderer les opérations de la nature, pourroient s'eclairer mutuellement. Je n'ai pas la folle préſomption de croire que je ferai aſſéz heureux pour faire beaucoup de chemin dans cette carriere obſcure, je me ſens ſeulement le courage d'y entrer.

have in view; but I ſhall be obliged to take up the ſubject far backwards. An accident has juſt now deprived me of my twelve little anemonies: the ſea-water was, in in the firſt days of March, ſo much troubled by the falling in of part of the cliff, &c. that my great anemonies of the firſt and third ſpecies have conſiderably ſuffered, and all the ſmall ones of the fourth ſpecies died in one day. Another accident had juſt preceded this, and had occaſioned the loſs of a whole year's labour: on the 28th of January, REAUMUR's thermometer fell to 15° below the freezing point; I had then forty vaſes containing anemonies on which I was making experiments, and was at the ſame time attending to other avocations: notwithſtanding all the meaſures I had taken to prevent it, the water froze in ſeveral of them: my anemonies however would not have died, ſince one of the firſt ſpecies, which I had on

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Je compte beaucoup ſur les expériences que je médite, mais je ſerai obligé de reprendre les choſes de loin. Un accident vient de me priver de mes douze petites anémones: l'eau de la mer, dans les prémiers jours de Mars, a été tellement troublée par l'eboulement des falaiſes, &c. que mes grandes anémones de la prémiere et de la troiſieme eſpece ont conſidérablement ſouffert, et les petites de la quatrieme ſont toutes mortes en un même jour. Un autre accident avoit précédé, et m'avoit fait perdre un an de travail: le 28 Janvier, le thermometre de REAUMUR deſcendit à 15° de condenſation; j'avois alors quarante vaſes d'anémones de mer en expérience, et mes ſoins étoient partagés par d'autres objets: quelque précaution que j'aye pris, l'eau de pluſieurs gela; mes anémones n'en ſeroient

on purpofe fuffered to freeze out of the water, did not perifh; but they fuffered a good deal, and I am determined to avoid all uncertainty in my experiments.

If we judge of the multiplication of the anemonies of the fourth fpecies, by the number of young ones that are feen round the large ones, it muft be very confiderable. This fpecies alfo affords us a fingularity which is not obfervable in the frefh-water polypi, that of multiplying by tearing off, of its own accord, fmall fhreds from its body.

Although the main object of this effay be the generation of the fourth fpecies, I cannot however omit obferving that feveral of its individuals, having been cut into two equal parts perpendicularly to their bafes, formed each of them two compleat anemonies. And I beg leave to infert here an obfervation on the firft fpecies, which

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feroient pas mortes, puifqu'une de la prémiere efpece, que j'ai fait geler exprès à fec, n'a point péri : mais elles ont beaucoup fouffert, et je ne veux aucun equivoque dans mes expériences.

A en juger par le nombre des petits qu'on trouve autour des groffes anémones de la quatrieme efpece, leur multiplication eft très nombreufe : elle offre auffi, comme on vient de voir, une fingularité que ne nous ont point prefenté les polipes d'eau douce, celles de fe multiplier en s'arrachant elles mémes des petits lambeaux.

Quoique ce mémoire ait pour objet la génération de la quatrieme efpece, je ne laifferai pas d'indiquer que plufieurs de fes individus, coupés en deux portions égales perpendiculairement à leur bafe, ont formé chacun deux anémones : et d'inferer

which will in ſome meaſure ſupply the want of an eſſay, the publication of which is retarded by the abovementioned accident.

A FURTHER OBSERVATION ON THE FIRST SPECIES.

TOWARDS the end of the year 1774, I cut in two, in a perpendicular direction, an anemony of the firſt ſpecies, which had been formed from a moiety of one I had cut before, ſo that each half was then only a quarter of the primitive anemony. Theſe two halves had the ſame fate as the firſt ſections; and one of them, after having been thus reſtored, and having been always kept by itſelf, produced, on the 1ſt of June 1775, a young anemony as perfect as thoſe that are produced at ſea, and of the ſame colour as the mother. It muſt be remembered that

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d'inſerer ici une notice ſur la prémiere eſpece qui ſuppléera en partie au mémoire dont les accidents que je viens de rappoter retarderont la publication.

NOTICE SUR LA PRÉMIERE ESPECE.

A LA fin de 1774, je coupai par la moitié, dans une direction perpendiculaire à ſa baſe, une anémone de la prémiere eſpece, qui après l'avoir été, s'etoit reformée: chaque moitié n'etoit donc alors que le quart de l'anémone primitive. Ces deux parties ont fait comme la prémiere fois; et une d'elles, après s'être ainſi reformée, et avoir toujours vécu en particulier, me donna, le prémier Juin 1775, une petite anémone, auſſi parfaite que celles qui naiſſent à la mer, et de la même couleur que la mere. Qu'on j'uge de ma ſurpriſe quoique je m'attende à tout; et ſi

that in this ſpecies, the young ones are entirely formed in the inſide of the animal, whence they are put forth through the mouth; ſo that, whatever idea we may form to ourſelves of this ſpecies of beings, we can find no real analogy between any ſections from them, and thoſe torn from ſtems and roots of certain trees, with a view to multiply them. The young anemony we are ſpeaking of is not large enough to favour the ſuppoſition that it had been ready for birth, in that part of the old anemony, for more than two years before; ſince ſome young anemonies of this ſpecies, which I had kept in order to obſerve their encreaſe, have in ten months time grown to double the diameter of their baſes, without my having ever taken the trouble to feed them: and it is beſides known, that it is uſual for theſe anemonies, when they are

ſi on me demande, comment ceci, comment cela? je répondrai ſans rougir (puisqu'on peut le faire maintenant) je n'en ſçais rien. On ſe reſſouvient ſans doute que, dans cette eſpece, les petits ſont entiérement formés au plus intérieur de l'animal, d'ou ils naiſſent plus ou moins gros par la bouche: ainſi on ne pourroit, quelqu' idée qu'on ſe fit de ces animaux, trouver d'analogie réelle entre ces ſections, et celles qu'on fait aux tiges et aux racines de certaines plantes pour les multiplier. La petite anémone dont nous parlons n'eſt pas aſſez groſſe pour qu'on puiſſe penſer qu'elle ait réſté prête à naitre depuis plus de deux ans dans cette partie de l'anémone primitive, puiſque des petits de cette eſpece, que j'avois précédemment gardés pour en obſerver l'acroiſſement, ont augmenté du double de leur diametre en dix mois, ſans que je priſſe la peine de les nourir; et on ſçait d'ailleurs

are put to any pain, to eject all the young ones they contain.

The reſtored moiety produced another young anemony on the 7th of Auguſt, another on the 27th, one more on the 1ſt of September, a larger one on the 20th of October; whereas to this day the other half has not yet afforded me a ſingle young one. Another circumſtance worth notice is, that the two halves of the original anemony never produced any young ones, neither during their recovery, nor after their reſtoration.

Some perſons, who intereſt themſelves in the progreſs of my experiments, induced me on the 27th of June laſt, to cut an anemony of the firſt ſpecies perpendicularly into four parts. For this purpoſe I choſe a very large one, juſt taken out of the ſea; and on cutting it in that manner, it put forth twelve young ones. One of theſe

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leurs qu'il eſt aſſéz ordinaire qu'une anémone, lorſqu'elle ſouffre quelque incommodité notable, pouſſe dehors les petits qu'elle renferme.

Revenons à notre moitié réformée : elle m'a donné une autre petite anémone le 5 Août, une le 27, une le 1 Septembre, enfin une plus grande que les autres le 20 Octobre: juſqu'au 7 Mars 1776, que j'écris ceci, l'autre partie ne m'a point encore donné de petits. Une choſe qui eſt à remarquer, c'eſt que les deux moitiés de l'anémone primitive, ni en ſe réformant, ni après s'être reformées, ne m'en avoient donné aucun.

Quelques perſonnes qui s'intéreſſent à mes expériences, m'inviterent le 27 Juin dernier, à couper, perpendiculairement à ſa baſe, une anémone de la prémiere eſpece, en quatre parties, comme j'en avois coupé en deux : j'en pris une aſſéz

 groſſe

theſe quarters adhered the next day to the ſide of the vaſe: on the 30th it had crept to the ſurface of the water. It looked healthy till the middle of November; but at the end of the year the wound was not yet perfectly healed: nevertheleſs, a few days after, it put forth a young one of a tolerable ſize. The cold of the 28th of January ſeems to have accelerated its diſſolution. The ſecond quarter had nearly the ſame fate, except that it yielded no young ones. The third produced, on the 6th of September, a young one of a very ſmall ſize. On the 15th the wound ſeemed to be cloſed, but its place ſtill appeared of a pale colour, tranſparent, and conſiderably depreſſed. On the 30th it put forth another ſmall young one; and on the 26th of October, a third of a ſomewhat larger ſize. On the 29th of January 1776, after the water had been often frozen, I ſaw it put forth three young

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groſſe qui venoit d'être péchée, et en la coupant ainſi, elle me donna douze petits. Une de ces portions s'attacha le lendemain aux parois du vaſe: le 20 elle etoit montée à fleur d'eau: elle a fait bonne figure juſqu'à la moitié de Novembre: à la fin de l'année la playe n'etoit qu'imparfaitement conſolidée; peu de jours avant, ſans avoir l'air vigoreux, elle avoit donné un petit un peu fort. Le froid du 28 Janvier a paru accélerer ſa ruine. Il en a été à peu près de même de la ſeconde portion comme de la précédente; mais elle n'a point donné de petits. La troiſieme m'a donné un petit d'une petiteſſe extréme le 6 Septembre. Le 15, la playe paroiſſoit conſolidée; mais la marque en etoit très ſenſible par la couleur pâle, la tranſparence, et un reſte d'applatiſſement. Le 30 elle m'a donné un petit très petit, et le 26 Octobre un un peu plus gros. Le 29 Janvier 1776,

young ones of a moderate ſize. On the 31ſt it produced a ſeventh; but ſince the froſt it has ever appeared in a weakly ſtate. The fourth quarter, after ſeveral changes in its ſtate of health, produced, on the 29th of November, three young ones; one large, the other of a middling ſize, and the third very ſmall. After the ſevere cold it declined: it nevertheleſs put forth three more young ones, and died one of the firſt days in March. Thus from three of theſe quarters I had no leſs than fourteen young ones, beſides the twelve the animal had produced during the operation. The interior contractions of the anemonies not only renders it difficult to cut them into four parts; but I have alſo obſerved, that thoſe ſections do not eaſily recover a cylindrical form, and that they are eaſily affected by froſt or any other accident. This therefore is an experiment of little uſe, which

1776, après que l'eau fut glacée pluſieurs fois, j'ai vû naitre trois petits de moyenne groſſeur. Le 31 Fevrïer elle m'en a donné un ſeptieme; mais depuis la gelée, elle a fait mauvaiſe figure. Après des alternatives de bon et de mauvais état, la quatrieme s'eſt réformée, et m'a donné le 29 Novembre trois petits, l'un gros, l'autre moyen, et le troiſieme très petit. Depuis le grand froid elle alloit mal; elle a cependant donné encore trois petits, et s'eſt décompoſée dans les premiers jours de Mars. Voici donc quatorze petits donnés par trois de ces portions, indépendamment des douze que l'anémone avoit rendûs en la coupant. Non ſeulement il eſt difficile de couper exactement une anémone en quatre, à cauſe des contractions intérieures; mais même j'ai remarqué que ces portions avoient bien de la peine à reprendre une forme cylindrique, et que les accidens comme la gelée et autres, les

which I only relate on account of the number of young ones; and to ſhew, that even a quarter of an anemony tends towards reſuming, and does ſometimes actually reſume the cylindrical form, which is the figure of the whole animal when it dilates itſelf.

les affectent beaucoup. C'eſt donc une expérience peu fructueuſe, que je ne rapporte qu'à cauſe des petits; et pour faire connoitre que juſqu'à un quart d'anémone tend, et peut même parvenir quelquefois à reprendre la forme cylindrique qu'à l'anémone entiere lors qu'elle ſe dilate.

VIII. *Experiments and Obſervations in Electricity. By Mr.* William Henly, *F. R. S.*

P A R T I.

Remarks on the effects of lamp-black and tar, or lamp-black and oil, as protectors of bodies from the ſtroke of lightning; with ſimilar effects produced by experiments in the artificial electricity.

Read, Jan. 16 and 23, 1777.

DR. LEWIS, in his Philoſophical Commerce of Arts, p. 364. mentions a remarkable inſtance of the effect of a coating of lamp-black and tar, in preſerving thoſe parts of the maſt of a ſhip, which were covered with it, from damage by a ſtroke of lightning which ſhivered the other, that is, the uncoated parts of it, in a very extraordinary manner. The account is recited at large in vol. XLVIII. of the Philoſophical Tranſactions. Captain NAIRNE, in his Remarks on the Effects of Lightning, on the maſts of ſeveral veſſels which were ſtruck in the Baſon at Quebec[a], mentions, in his letters to ſome friends in London, no leſs than five inſtances, where the lightning paſſing

(a) His own ſhip, the *Generous Friends*, was twice preſerved there by his conductor.

over

over thoſe parts of the maſts of the ſhips which were covered with lamp-black and tar, or painted with lamp-black and oil, without the leaſt injury, ſhivered the un-coated parts (tearing out ſplinters five or ſix feet in length, and ſix or eight inches deep) in ſuch a manner as to render the maſts entirely uſeleſs. A very curious inſtance of this kind hath lately been communicated to me by a learned and ingenious member of the Royal Society, from whom I received the account which I ſhall here inſert *verbatim*,

"On the firſt of April, 1776, being on board a brig "in the latitude 34° N. our veſſel was ſtruck with lightning, which diſabled our fore-maſt. Upon getting the "whole of the top-maſts down upon the deck, I ob-"ſerved the following particulars, with regard to the "courſe and effects of the electrical matter. The light-"ning had firſt ſtruck the pinnacle of the fore-top-gal-"lant-maſt (on which, at that time, there happened to "be no iron ſpindle, as uſual) which it juſt ſplit ſlightly "for about two feet and a half, where it was painted "with lamp-black and oil (I could not, however, aſcer-"tain whether this part of the maſt had been newly "ſplit or not: it might have been firſt occaſioned by "driving in the iron ſpindle, whereon a vane is uſually "placed; for there was a hole where ſuch a ſpindle had

"been

"been fixed) and then the lightning immediately shi-
"vered the rest of that mast as far as it was greased, till
"it met the bottom of that and the top of the top-mast,
"which had likewise been coated over with lamp-black
"and oil, on an old coat of tar. Over these it glanced,
"without any perceptible effect, till it reached that part
"of the top-mast which was also greased; this part it shi-
"vered, splitting off large splinters which were thrown
"on the deck. The lower part of the fore-mast being
"tarred, the lightning slid over it for about nine
"feet, without the least mark of violence; then darted
"into the fore-mast, where it was greased; rived it ter-
"ribly, tearing off splinters as large as a man's arm, and
"four or five feet long, all the way down. It continued
"this course till it again met a coat of tar, which was
"laid on the mast for more than five feet above the
"deck; here its effects on the mast disappeared, and its
"course seemed to be divided different ways by two at-
"tractive bodies of iron; one, in a cleet fastened to the
"fore-mast, about ten inches below the top of the last
"mentioned coat of tar, by two large spike-nails: this
"cleet with the nails was entirely struck off; the light-
"ning then tore the canvas coating round the foot of
"the mast (about a yard below the cleet) without any
"other hurt to the mast itself; and from thence was

"attracted

" attracted by a large anchor lying in a horizontal posi-
" tion on the deck, about two feet from the mast. All this
" part of the electrical matter seems to have been then
" discharged by the different conductors of the two flukes
" and the main shank of the anchor, without any other
" visible effect than breaking a piece out of a large iron
" pot, standing in an immediate direction to one of the
" flukes, and about ten inches distant. The other divi-
" sion of the electrical matter darted from the mast to
" the belfry, about fifteen inches distant, tore off two
" cleets fastened to it by large iron nails; then descended
" to the large iron hinges which hold the palls or stops
" (joined to the belfry-posts) of the windlass, where,
" after tearing off a small nail or two, it totally disap-
" peared. The belfry which was painted was much
" split, but not broke to pieces. The most remarkable
" part of this accident, and for which the whole relation
" is given, is, the effect of the tar and lamp-black and
" oil as repellents of the electrical matter; for in four
" instances, 1st, at the head of the fore-top-gallant-mast;
" 2dly, at the bottom of that and head of the top-mast;
" 3dly, at the bottom of the top-mast and head of the
" fore-mast; and 4thly, at the bottom of the fore-mast;
" the lightning had glanced over all those parts without
" doing the least damage, and had regularly darted into,

" and

"and ſhivered all the intermediate parts of thoſe maſts, "where they were covered with greaſe. This appears "ſo extraordinary to me, that I thought proper to take a "note of it upon the ſpot, in order to occaſion the making "of ſome philoſophical inquiries and experiments on "the ſubject by thoſe more capable of accounting for it "than myſelf: and for that reaſon the above relation "may be depended on as juſt and true, after a repeated "and critical examination of the ſeveral parts of the "maſts which I have deſcribed. W. D."

In conſequence of this relation, and the hint which concludes it, I have been induced to make the following experiments. 1ſt, A glaſs tube, eight inches long, with a bore or cavity of about a quarter of an inch diameter, being filled pretty cloſely with lamp-black, and each end ſtopped with a bullet, the ſtem of which juſt entered, and was cemented in the cavity of the tube, conducted the charge of a jar containing three ſquare feet of coated ſurface inſtantaneouſly, but with ſcarce any exploſion. 2dly, Such a tube being filled with a mixture of lamp-black and oil (as uſed by the painters) entirely failed to conduct the ſhock. 3dly, The outer ſurface of ſuch a tube being painted with lamp-black and oil, and excited with dry warm flannel, acted (the tube being alſo dry and warm) as a very ſtrong negative electric. 4thly, A piece

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of polifhed plate glafs being introduced into the circuit, the ends of the wires which compofed the circuit were laid at about an inch and an half from each other upon the furface of the glafs, when the jar above-mentioned being difcharged, the polifh of the glafs was always torn off in an irregular and deep line, extending from one wire to the other; but if a piece of glafs painted with lamp-black and oil were thus introduced into the circuit, and the difcharge made as before, not the fmalleft trace of the electricity could ever be perceived on its furface. 5thly, If inftead of the glafs, a flip of writing-paper was introduced into the circuit, it was torn in pieces by the explofion, much of it flying about the room in the form of fine flue or down. 6thly, A flip of the fame paper, painted with lamp-black and oil, received not the leaft injury, nor fhewed the fmalleft trace of the electricity upon its furface; but the common oiled paper, without lamp-black, was torn confiderably, though not fo much as the paper unoiled[(b)]. When the charge of the jar above-mentioned is made to pafs between the furface

(*b*) Mr. CAVALLO, who hath fince repeated thefe experiments, finds, that if the paper be very thinly painted with the lamp-black and oil, it will be torn by the explofion; but having tried a piece of the fame that I had ufed in my experiments, he was not able to make the leaft impreffion on it. Lamp-black and tar therefore, on account of the greater tenacity of the latter, and its being equally a non-conductor, feems to be the moft proper for the purpofe.

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of thick plate glafs, and that of a cylinder of ivory three-quarters of an inch diameter, preffed by a weight of about fix or eight ounces Troy; the glafs is always fhivered into very fmall fragments, and part of it is fometimes reduced to an impalpable powder. But 7thly, If the plate of glafs be covered with a flip of writing-paper, painted with lamp-black and oil, or with a flip of oiled filk (fuch as is frequently ufed for garments) the charge paffes over thefe fubftances without leaving the fmalleft trace on either of them, though the glafs under them be broken by the blow of the explofion.

From thefe experiments, and the obfervations above recited, I think the following corollaries may be deduced. 1ft, That a charge of electricity, or a ftroke of lightning, which is the fame thing, paffes, in many cafes, upon the furface of bodies, in a much larger proportion than through the interior fubftance of them, as appears by the mafts of fhips, coated with lamp-black, &c.[c], and by the experiment above recited, with the cylinder of ivory and the glafs &c.; for in this experiment, the charge being refifted by the ivory (which however is fometimes fplit by the explofion) forces a paffage between that and the glafs, and being there confined by heavy weights, exerts

(c) See a curious inftance of this kind in M. ADANSON's Voyage to Senegal, p. 239.

its expanſive power in ſuch manner as to reduce to the ſmalleſt fragments the plate of glaſs then expoſed to its violent operation. 2dly, This violent effort of the electricity produces not the leaſt effect upon the ſurface of the ſlip of paper painted with lamp-black and oil, or upon a ſlip of oiled ſilk, placed in the ſame ſituation. 3dly, May not therefore a coating of lamp-black and tar, or lamp-black and oil, be in ſome caſes uſefully applied on ſlight buildings of wood, &c. to preſerve them from damage by lightning, as well as to prevent thoſe large cracks and rents (the uſual effect of the heat of the ſun) from being made in them? 4thly, As the effect of the lightning on the maſts of ſhips has been in ſo many inſtances prevented by a coating of lamp-black and tar, or lamp-black and oil, it ſeems probable, that a ſafe and fixed conductor might be applied to them in a very cheap and convenient manner, as follows; *viz.* let all thoſe parts of the maſt which are uſually greaſed, be provided with plates of metal three inches broad, which plates might extend a few inches upon the other parts of the maſt which are coated with lamp-black and tar, or lamp-black and oil; and thus by the conductor of metal, and the protector of lamp-black and tar, placed alternately and extending the whole length of the maſt, it would probably be preſerved from damage by lightning. A

metallic

metallic communication might be made from the maſt to the water in the manner I have before mentioned, in Phil. Tranſ. vol. LXIV. p. 412. This method of making conductors to ſhips, from its ſimplicity and practicability, I had ſome thoughts of recommending to my acquaintance in the marine department; but there is one objection to it, which I think a very material one, and ſhall therefore ſtate it in its full force: it is this; the lamp-black and tar, or lamp-black and oil, though they protect, by their property of repelling the electric matter, thoſe parts of the maſt which are coated with them, yet being perfect non-conductors, thoſe things or perſons which might happen to ſtand in their vicinity (as in the tops, &c.) would be in danger of a ſevere ſtroke, perhaps deſtruction, by the lightning. How far the other oil colours, *viz.* thoſe prepared from minerals and metals, may anſwer theſe purpoſes, may perhaps deſerve enquiry, and the more ſo, as the experiments are not difficult to make. The belfry-poſts painted with white lead, mentioned in the letter above recited, were much ſhivered. 5thly, As oiled ſilk ſeems to be ſo good a ſecurity againſt the effects of a charge of electricity, may not garments, *viz.* cloaks and hats, covered with that ſubſtance, contribute in ſome meaſure to protect the wearers (if overtaken by a ſtorm) from a ſtroke of lightning? particularly, if the precau-

tions be properly attended to, which I have before recommended from Dr. WINTHROP, in Phil. Tranf. vol. LXIV. p. 151.

PART. II.

On the electricity of chocolate: and the restoration of that property to it, when lost, by melting it together with a small quantity of olive-oil.

HAVING been informed by my ingenious friend Mr. GEORGE ADAMS, philofophical inftrument-maker to his majefty, that Mr. SANDERS, an eminent manufacturer of chocolate, had frequently obferved a very vivid light flafhing upon its furface, when cooling in the tin pans in which it is received from the mill; particularly in clear, frofty evenings, when it would alfo ftrongly attract light fubftances, fuch as fmall particles of duft, bits of paper, ftraw, thread, &c.[d]; I was very defirous to afcertain, if I could, the caufe of thefe phenomena. For this purpofe I waited on Mr. SANDERS, in company with Mr. ADAMS, and made the following experiments. 1ft, A large cake

(d) The wax-chandlers alfo, in forming their fticks, &c. of wax, are frequently fpectators of thefe effects of electric attraction.

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of chocolate being turned out of the tin pan, in which it had been ſet to cool; I preſented towards it Mr. CANTON's electrometer, and obſerved that, at ſix inches diſtance, the balls began to diverge; and when they came within two inches of the chocolate (being ſuſpended over it) their divergence was full an inch and an half, and upon examining their electricity, I found the chocolate to be in a *plus* or poſitive ſtate. 2dly, Having ſeparated another large cake from the pan, I touched it repeatedly with the knob of a ſmall phial, properly prepared for the Leyden experiment; then bringing that knob gently toward one of my knuckles, I ſaw a ſpark between them, and had a ſmall ſenſation in my knuckle. 3dly, Having ſeparated another cake of chocolate from the tin pan which contained it, I touched the cake repeatedly with the braſs ball on the neck of my Leyden *vacuum*, or analyſis of the Leyden bottle[(e)], and inſtantly perceived a moſt beautiful and large pencil of rays darting from the wire, and ſpreading themſelves through the bulb towards the coated part of the bottle. 4thly, Changing the poſition of the bottle, I preſented the coated bulb towards the chocolate, and then perceived (as I expected) a ſmall luminous ſpark upon the point of the wire in the neck of the bottle; completely proving the electrical quality of the

(e) Deſcribed in Phil. Tranſ. vol. LXIV. p. 400.

chocolate,

chocolate, and ascertaining its direction in the experiments.

Before I had an opportunity of making this complete investigation, I had separated a piece of chocolate from the tin pan in which it had been cooled, and accidentally left, for some months, under an open counter in a shop, exposed to dust, damp air &c.; notwithstanding which, on its separation from the pan, it attracted a thread of trial at a quarter of an inch distance. I then took a quarter of a pound of chocolate, and having melted it in an iron ladle, poured it into a tin pan, and the next day (it being perfectly cooled) separated it from the pan, and found it strongly electrified *plus*; but as the electricity was soon lost by handling (owing, I suppose, to the large quantity of conducting matter contained in it) I melted it again, but produced no electricity; which I imputed to the chocolate having become very dry and powdery. I therefore melted it a third time, adding a little oil of turpentine; but this trial also (perhaps from the evaporation of the spirit) failed. I then melted it the fourth time, and added a small quantity of olive-oil, sufficient as I imagined to reduce it again to its original consistence, and having cooled it in the tin pan as before, I found on removing it, that its electricity was completely restored. The large proportion of phlogiston in oil is well known; and as the addition of oil to the chocolate completely

completely reſtored its electricity when loſt, is not this an indication of a great affinity at leaſt between phlogiſton and the electric fluid, if indeed they be not the ſame thing[(f)]? Further, as electricity is produced in the chocolate by heat and friction, and manifeſted by its uſual phenomena in the cooling of that ſubſtance, *query*, may not electricity be produced from the other oily nuts, kernels, or ſeeds (particularly thoſe of the torrid zone) treated in the ſame manner?

However, as the electric matter is reſident in, and may be diſengaged from, all the ſubſtances we are acquainted with; as the air is at all times replete with it; as its opeation is ſo ſecret, ſo rapid, and at times ſo tremendous; as it is ſo eaſily excited or put in action by friction, by heating and cooling, and perhaps by means we are totally unacquainted with; I think we may ſafely conclude, that electricity, as it is one of the moſt powerful, is alſo one of the moſt important, agents in nature. Many uſeful diſcoveries have been made reſpecting the action, influence, and effects of this ſubtile fluid; but certainly much remains to be done, and the field for future labourers ſeems daily to enlarge. Indeed, notwithſtanding the number of diſcoveries in electricity this age may juſtly boaſt of, I cannot but be of opinion (which I men-

(*f*) A thick ſcum from the ſurface of ſome linſee-doil expoſed to the air, and thoroughly dried, became a very ſtrong negative electric.

tion as an incitement to the ſtudy) that, compared with the facts ſtill undiſcovered in that branch of philoſophy, they bear but a very ſmall proportion.

PART III.

Obſervations on ſome new and ſingular phenomena in excited and charged glaſs; with experiments made in conſequence of theſe phenomena, further illuſtrating the Franklinian theory of the Leyden bottle; and a deſcription of the apparatus conſtructed for that purpoſe by Mr. HENLY.

HAVING carefully repeated the experiments with the two coated plates of looking-glaſs, mentioned in my paper on Mr. VOLTA's machine, and finding with Mr. LANE, that they exactly agreed with the account given by Mr. SYMMER and Mr. EELES, I was deſirous to be ſatisfied whether glaſſes of a different thickneſs would be differently affected in the experiment. For this purpoſe I tried two large ſquares of crown or window-glaſs, and found them to charge and diſcharge exactly as the looking-glaſs plates had done; but on trying the experiment with two plates of Nuremburg glaſs, commonly called Dutch plates, I was not a little ſurpriſed to find that each

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of the plates, when ſeparated after charging, had a poſitive and a negative ſurface; and that having replaced them, and made the diſcharge as in the Leyden experiment, the electricity of all the ſurfaces was changed, though it appeared to be very ſtrong, and continued to give repeated flaſhes of light, when the plates were alternately cloſed, touched, and ſeparated, as the looking-glaſs plates above mentioned. If a clean dry uncoated plate of looking-glaſs was placed between the coated plates, either of looking-glaſs or crown-glaſs, before they were charged; that uncoated plate was always found, upon ſeparating them after charging, to be electrified negatively on both its ſurfaces; but if it was put between the Dutch plates, it acquired, like them, a poſitive and a negative electricity. As this phenomenon was not ſatiſfactorily accounted for, it occaſioned much converſation with reſpect to Dr. FRANKLIN's theory of the Leyden bottle, which I had myſelf (as I imagined) ſatisfactorily explained and even demonſtrated. I was, however, ſoon convinced, that that theory is not ſo generally received as I imagined; for I met with a number of gentlemen who not only doubted, but ſeemed abſolutely to deny it. This induced me to make ſome further experiments, in order (if I could) more fully to illuſtrate that theory, and to put the matter out of doubt. For

this purpofe a pretty large jar was coated and furnifhed as in fig. I. A is the jar; BB the tin-foil coating; C a tin-ftand which fupports the jar; D a focket of metal which fupports a rod of glafs E; F a curved wire or plate of metal with points, not very fharp; this wire or plate of metal is faftened to the end of a brafs rod G, which rod is moveable at pleafure in a fpring tube H, that tube being fixed by a focket upon the top of the glafs rod E. The charging wire of the jar communicates with both parts of the infide coating of the jar by horizontal wires (the ends of which are bent a little downwards) fixed at right angles to each other, in order to prevent fhaking and ratling.

THE USE OF THE DOUBLE COATED JAR.

According to Dr. FRANKLIN's theory, the fame quantity of the electric matter which is thrown upon one of the furfaces of glafs in the operation of charging it, is at the fame time repelled or driven out from the other furface, and thus one of the furfaces becomes charged *plus*, the other *minus*; and that this is really the cafe is, I think, fatisfactorily proved by this contrivance. For example, place the jar as ufual, with the knob in contact with the prime conductor; then work the machine, and the apparatus being perfectly dry and in good order, a fmall luminous fpark will appear upon the upper point of the wire F (a plain indication that the point is then receiving

electricity from the upper ring of coating on the outſide of the jar) and a fine ſtream or pencil of rays will at the ſame time fly off, beautifully diverging from the lower point of the wire F upon the bottom ring of coating on the jar. When theſe appearances ceaſe, which they will as ſoon as the jar becomes charged, let a pointed wire be preſented towards the prime conductor; this will ſoon diſcharge the jar ſilently, during which the lower point of the wire F will be illumined with the ſmall ſpark, while the upper point of the wire will throw off a fine pencil of rays, diverging towards the upper ring of coating, to which it ſtands contiguous, as upon Dr. FRANKLIN's hypotheſis it ought to do. A wire of the ſame form as that marked F may be inſerted on a ſmall electric ſtand, fitted by a proper baſe to the bottom of the jar on the inſide; this will ſhew the appearances when the jar is charged negatively.

The ſame experiment may be very conveniently made with a large pane of crown glaſs[(g)], coated in two places at a proper diſtance from each other (fig. 2.) leaving a ſufficient quantity of glaſs uncovered quite round the two coatings. This coated plate of glaſs ſhould be fixed in a frame, and mounted upon a proper electric ſtand. Another ſtand of glaſs or ſealing wax ſhould be provided,

(g) Mine is eighteen inches by fourteen inches.

to ſupport a wire or piece of metal placed horizontally, and curved ſo as to bring the ends of it, which ſhould have blunt points, within half an inch diſtance of the two tin-foil coatings on one of the ſurfaces of the glaſs. On the oppoſite ſide of the glaſs, two wires, bluntly pointed, are alſo to be employed; one of theſe is to communicate with the prime conductor, and to throw off the electricity from thence upon one of the coatings of tin-foil placed contiguous to it; the other wire is to communicate with the earth, ſtanding in a perpendicular direction, with the point bent towards and reaching within half an inch of the other coating of tin-foil (on the ſame ſurface of the glaſs) to receive the electricity thrown off by that coating, while the oppoſite ſide is charging.

EXPERIMENT.

The apparatus being perfectly dry (the uncoated part of the glaſs and the frame, &c. ſhould be varniſhed), clean, and in good order; the plate of glaſs ſhould be ſo fixed, that each of the four coatings of tin-foil may come within half an inch of the point of the wire oppoſed to it. The apparatus being thus placed, if a powerful machine be worked in a dark room, the electricity will be ſeen to iſſue from the point of the wire in contact with the prime conductor upon one of the tin-foil coatings A

(fig.

(fig. 3.) charging it poſitively. The coating B (fig. 3.) on the other ſide of the glaſs throwing off, at the ſame time, an equal quantity of the electric matter, (viſible in the form of a ſmall luminous ſpark upon the point of the inſulated wire F) is thus left in a negative ſtate. The electricity paſſing along the inſulated wire, flies off from the other point of it in a pencil of rays, diverging upon the tin-foil coating C (fig. 2.) on the ſame ſide of the glaſs, charging it poſitively; while the oppoſite coating D (fig. 3.) throws off its electricity, which is received in a ſmall ſpark upon the point of the wire (G) oppoſed to it, and communicating with the earth. Thus, by the ſame operation of the cylinder, may a poſitive and a negative charge of electricity be obtained at the ſame time upon each ſurface of the glaſs; and by applying two curved diſcharging rods (which ſhould have glaſs handles) at the ſame inſtant, ſo as to come nearly into contact with the coatings upon each ſurface of the glaſs, the whole will be diſcharged together; or if a pointed wire be preſented near to the prime conductor, they will all be diſcharged ſilently, and then the appearances on the points of the wires will all be reverſed; that which was a bruſh or pencil of rays being now a ſmall luminous ſpark, and that which was a luminous ſpark being a bruſh or pencil of rays. If the machine be very powerful, the rubber

rubber may be inſulated, and a blunt pointed wire, communicating with the earth, may be placed within half an inch of it; this wire, while the plates are charging, will throw off a beautiful pencil of rays diverging upon the rubber, and thus compleatly exhibit the progreſs of the electricity through all the apparatus, from its exit out of the earth to its entrance into the earth again: and its return may be manifeſted by reverſing all the appearances upon the points of the wires, in the operation of diſcharging the glaſs ſilently by a pointed wire preſented toward the prime conductor, as above directed. Another very ſatisfactory method of demonſtrating the truth of Dr. FRANKLIN's hypotheſis is as follows. I take a bottle, containing about one hundred ſquare inches of coated ſurface, properly prepared for the Leyden experiment, and holding it by the wire, I ſet the coating upon the prime conductor, and charge it negatively (fig. 6.); when charged (if not too dry) the upper edge of the coating will throw off one or more pencils or bruſhes of light into the air, which viſibly incline towards the charging wire of the bottle, and ſometimes actually reach it. If I hold the bottle by the coating, and preſent the knob to the prime conductor, charging it poſitively (the bottle being in a proper ſtate) a ſmall ſpark of light firſt appears upon the edge of the cork in the neck of the bottle, through

which

which the wire paſſes; after a few turns of the globe, this ſpark becomes a fine bruſh, darting out from the cork, and gradually lengthening, till it forms a beautiful arch, the end of it regularly extending downward, till it reaches the edge of the coating and reſts upon it (ſee fig. 5.). I remember, when I firſt ſhewed theſe experiments to my ſincerely reſpected and worthy friend the late ingenious Mr. FERGUSON, F. R. S. he expreſſed great ſatisfaction; and aſſured me, that he thought them ſome of the moſt convincing he had ever ſeen exhibited for the purpoſe. If the bottle be dry, it will, in both caſes, be diſcharged ſpontaneouſly; but if the uncoated part of the glaſs be then breathed upon, the appearances may be produced at pleaſure. I have lately prepared another bottle for this purpoſe, the inſide of which is coated in the uſual manner; but the outſide is covered with ſquare pieces of tin-foil about a quarter of an inch broad, and about three-ſixteenths of an inch diſtant from each other; the bottom is compleatly covered with the coating (fig. 4.). If in charging this bottle, the electricity paſſed abſolutely through the glaſs, it would find a ready conveyance by the coated bottom into the table (and then indeed it could never be charged at all); but the truth is, that this bottle does not become charged till ſtrong flaſhes of electricity have paſſed,

 diverging

diverging in different directions quite round it. If it be discharged by bringing a pointed wire near the wire, or the prime conductor, with which it is in contact, the noise it occasions much resembles the report of a fired cracker; and the uncoated glass between the spots of tin-foil is very brilliantly illuminated. If the bottle discharges itself spontaneously, or be discharged suddenly, by making a regular communication by the rod between the two surfaces of the glass, the whole outside surface seems to be illuminated. To produce these appearances the glass must be thoroughly dry.

EXPERIMENT.

A smooth piece of mahogany, two inches square and five inches long, was hollowed into an elliptic groove, about three-quarters of an inch deep, and painted with lamp-black and oil. Into this groove two wires, terminated by brass balls each three-quarters of an inch in diameter, were introduced; the brass balls being placed about one inch and an half from each other: between the brass balls, at an equal distance from each, was placed a ball of the pith of elder, half an inch in diameter, nicely turned in a lathe. The apparatus being thus adjusted, and the circuit compleated by a short chain, a bottle, containing forty square inches of coated surface,

was many times diſcharged through it; and whether the bottle was charged poſitively or negatively, the pith-ball conſtantly moved in the direction of the fluid, according to Dr. FRANKLIN's hypotheſis. This is a delicate experiment; but, as I have perfectly ſucceeded in it, I thought it proper to give this ſhort account of it. Perhaps if the pith-ball were ſuſpended by a ſilk ſtring, it might anſwer the purpoſe as well; but this I have not tried.

EXPERIMENT.

In melting ſmall wires ſome inches in length, I have often obſerved the wire to become red-hot, firſt at that end in contact with the diſcharging rod, and the redneſs has proceeded gradually and regularly towards the coating of the jars or battery; plainly and fully demonſtrating the direction of the electric matter in the diſcharge of the jars or battery, which, for this experiment, were always charged poſitively. This phenomenon hath alſo been obſerved by Mr. BELL, and many times by Mr. NAIRNE.

EXPERIMENT.

Before I quit the ſubject of the Leyden bottle, I ſhall mention one experiment more which I have lately made, and which gives a full and compleat anſwer to a paragraph in Dr. PRIESTLEY's Hiſtory of Electricity, 2d edit, p. 465. l. 21. It is this: I procured ſome phials coated with ſil-

ver, by burning it into the very ſubſtance of the glaſs, in ſuch a manner that nothing can remove it without injuring the glaſs together with the metal. Glaſs thus coated and burniſhed has certainly a moſt elegant appearance, has no inequalities or points upon its ſurface, and charges as high and as readily as when it is coated with tin-foil; ſuch glaſſes will diſcharge ſpontaneouſly, and one of them, which was very thin, was burſt by the exploſion; an accident which, by this double annealing, I was in hopes to have prevented, but was ſorry to find myſelf diſappointed.

OBSERVATIONS ON EXCITED GLASS.

In my remarks on Mr. VOLTA's curious little machine lately preſented to the Royal Society, I have obſerved that the electric matter, being once thoroughly excited and put in action, is not ſo ſoon as might be expected reduced again to a quieſcent ſtate, eſpecially in bodies ſo peculiarly adapted to affect each other as theſe appear to be. As a proof, I introduced the experiments with the looking-glaſs, crown-glaſs, and Dutch plates. I alſo mentioned Dr. PRIESTLEY's obſervations on the *reſiduum* of his battery; and in a note to that paper, I added an experiment made by my friend the rev. Mr. HEMMING, F. R. S. who ſhewed me a ſmall bottle, which then attracted a thread of trial, though it had ſtood in a cup-

I board

board in his ſtudy ſeventy days from the time he charged it. I alſo mentioned a ſingular effect of his cylinder, which will ſeparate the balls of Mr. CANTON's electrometer, at twelve or fourteen inches diſtance, ſometimes a fortnight after uſing, though the air of the room may have been many times changed, and a variety of methods uſed to deſtroy that power in the interval. As the detail is curious, I ſhall here inſert one ſet of experiments, as I find them regiſtered in a journal, which Mr. HEMMING took the trouble to make for my ſatisfaction. On May 13 1776, the cylinder was uſed, and when placed in the cupboard at 10 o'clock A. M. it ſeparated the balls at ſeven inches diſtance. The power was then entirely deſtroyed by breathing on it, and the electricity marked 0. From that time the journal proceeds as follows:

Day.	Hour.	Diſtance at which the balls diverge.
May 13	11 A. M.	7 inches.
14	8 A. M.	13
Breathed on it once,		9
Ditto four times,		8
Door open till	$9\frac{1}{2}$ A. M.	5
14	3 P. M.	7
	7 P. M.	6
	10 P. M.	$6\frac{1}{2}$

Breathed

Day.	Hour.	Diſtance at which the balls diverge.
Breathed on it twice,		0 inches
May 14	10½ P. M.	6
15	8 P. M.	13 wind N.
Door open ten minutes,		8
	9 P. M.	13
	10 P. M.	8
The power deſtroyed by flame,		0
16	7 A. M.	14½ wind N.
18	8 P. M.	17 wind N.
	11 P. M.	7½
Deſtroyed by flame,		0
20 A fire in the room the whole day.		
	4 P. M.	2
	8 P. M.	1
	10 P. M.	3
21	7 A. M.	9
	9 P. M.	9
Deſtroyed by flame quite round		0
22	7 A. M.	8½
	10 A. M.	11½ wind N.
Applied flame quite round the cylinder,		0
	1 P. M.	12½

May

Day.	Hour.	Distance at which the balls diverge.
May 22	4 P. M.	$12\frac{1}{2}$ inches.
	8 P. M. {flame quite round.}	0
	$10\frac{1}{2}$ P. M.	2

23d, Mr. HEMMING shewed me the experiment, when I saw the balls separate at twelve inches distance from the cylinder. The cause of these phenomena is, no doubt, the excited electricity lodged in the pores of the glass acting upon the vapour in the air of the room, and producing a greater or less effect, as circumstances may contribute to increase or diminish its operation. The cylinder was now used again, which closed this set of observations. Mr. HEMMING has added a meteorological journal for the time; but the particulars of this I did not think it necessary to introduce in the above extract.

A SECOND SET OF OBSERVATIONS.

Feb. 3, 1777, the cylinder was excited, and from the 5th to the 14th no fire had been made in the study.

Day.	Hour.	Distance at which the balls diverge.
14 (b)	3 P. M.	7 inches.
	10 P. M.	0
15	9 A. M.	11

(b) This was the first time, since August or September, that I had observed my cylinder to retain its attractive power longer than twelve hours after being excited, though it was constantly kept in the same place, and, as well as I can observe, under the very same circumstances.

Feb.

Day.	Hour.	Diſtance at which the balls diverge.
Feb. 15	4 P. M.	8 inches.
	9 P. M.	0
	10 P. M.	0
A large fire in the ſtudy the whole day.		
16	9 A. M.	9
	12	9
	8 P. M.	0
	10 P. M.	0
17	9 A. M.	3
	6 P. M.	2
	11 P. M.	0
18	8½ A. M.	2
Abſent till 22d,	3 P. M.	8
	6 P. M.	2
	7 P. M.	0
	11 P. M.	0
23	8 A. M.	7
	2 P. M.	2
	5 P. M.	0
	9 P. M.	0
	11 P. M.	0
24	9 A. M.	0
	12	0
	3 P. M.	0

Feb.

Day.	Hour.	Diſtance at which the balls diverge.
Feb. 25	9 A. M.	2 inches.
Abſent till 28th,	3 P. M.	2
	9 P. M.	0
Mar. 1	9 A. M.	0
The fire in the ſtudy put out at noon.		
	10 P. M.	0
2	9 A. M.	0
	1 P. M.	0
Abſent till 8th,	3 P. M.	$9\frac{1}{2}$
	6 P. M.	4
	8, 9, 10 P. M.	0
9	8 A. M.	9
	7, 8, 10 P. M.	0
10	8 A. M.	8

The cylinder was now excited again, which cloſed this ſet of obſervations. Theſe changes in the electricity ſeem very extraordinary, and I think them not eaſily to be accounted for, as they happened in ſtates of the weather which were totally different. I regret, however, that an accurate and ſenſible hygrometer was not obſerved the

whole time; and for ſuch a purpoſe I ſhould recommend Mr. COVENTRY's, made with a number of circular pieces of iſſue-paper, amounting to a certain weight, thoroughly heated and ſtrung on a thread, kept ſeparate from each other by a ſmall glaſs bead, and ſuſpended on one of the ends of a lever, nicely poized, and turning freely on its axis; the other end ſerving as an index to a graduated ſcale, on which it ſhews the weight of the moiſture imbibed at any time by the papers. This hygrometer, from its extreme ſenſibility, I ſhould chooſe, I ſay, to recommend for this purpoſe, in preference to any other that I have ever ſeen.

HAVING lately had occaſion to ſhew the experiments with the coated Dutch plates to an excellent electrician (Mr. CAVALLO) and having charged them as high as I could, and ſeparated them I think rather more expeditiouſly than uſual, I was aſtoniſhed to find, that the very ſame plates I have ſo often mentioned were now charged, the one poſitively, the other negatively, on both ſurfaces. I then laid them together, and having made the diſcharge as uſual, I ſeparated them, and found one of the plates negative on both ſides, and the other plate poſitive on one ſurface, and negative on the other. Here was a new cauſe of admiration, and I was utterly at a loſs to account for it, as the plates had in every inſtance be-

fore

fore uniformly acted as reprefented in my paper. At length I recollected, that this experiment had been made rather more expeditioufly than ufual: I therefore repeated it, and having allowed fomewhat more time between the removal of the plates from the prime conductor, and the feparation of them, in order to examine their electricity, I found on each plate a pofitive and a negative furface; and having replaced them and made the difcharge, I obferved that the electricity of all the furfaces was changed. I have mentioned this circumftance, to fhew how fmall a difference in the manner of making an experiment, will make an effential difference alfo in the refult. There is fomething, however, very fingular in this kind of glafs, which I believe is owing to its not being properly annealed; for I once met with a plate of it which I found very difficult to charge at all; and when a fmall quantity of electricity had been forced into it, it diffipated proportionably fooner, without the ufe of the difcharging rod.

In glafs, properly annealed, whether in the form of plates or jars, prepared for the Leyden experiment, the diffipation of the electricity is, in fome ftates of the atmofphere, a remarkable, and fometimes (when there is not a fire in the room) a difagreeable circumftance: this effect, however, in the jar itfelf, may be in a great meafure prevented, by having the uncoated part of the glafs

neatly covered with the beſt varniſh[g]; and I ſhould not omit to obſerve, that Mr. HEMMING's bottle, which retained its charge ſo long, was prepared in this manner.

POSTSCRIPT,

Containing ſome experiments and obſervations on Mr. VOLTA's *machine, by* Mr. TIBERIUS CAVALLO, *with remarks by Mr.* HENLY.

Mr. VOLTA's machine, which occaſioned ſeveral of the preceding enquiries, hath lately been made by Mr. CAVALLO, by coating the glaſs plate (about ſix inches in diameter) with ſealing-wax. With one excitation of this plate he ſoon charged a bottle compleatly, and with that charge pierced three holes in a card, which he hath ſince ſhewed me. If, when this machine acted vigorouſly, he inverted the excited plate, and ſet the braſs plate upon the glaſs, he produced a contrary electricity, but in a much ſmaller degree. If when the ſealing-wax was ſtrongly excited, ſo that ſparks, two inches long, might be drawn from the braſs plate, the excited wax, &c. was placed on an electric ſtand, and the proceſs continued as uſual; the ſparks from the braſs plate preſently dimi-

(g) The varniſhing ſhould be ſeveral times repeated.

niſhed,

nifhed, and in a fhort time almoft totally ceafed: this, I think, clearly indicates that the electricity in the lower furface of the glafs and the table were mutually affected in the operation, as well as that, in the excited fealing-wax and the brafs plate[(h)]. I have feen one of thefe machines, made by Mr. CAVALLO, act fo ftrongly that, upon feparating the brafs plate from the fealing-wax, a flafh has ftruck from the brafs toward the table, and it has befides given a ftrong fpark upon the knuckle, when held at upwards of an inch diftance. If the brafs plate, after being raifed from the wax, be prefented with its edge toward the wax (lightly touching it) and thus drawn over its furface, the electricity of the plate, he obferves, will be abforbed by the fealing-wax, clearly fhewing the ftrong negative ftate in which the excited wax is left on the removal of the brafs plate.

The attraction between the plates is alfo fometimes fo ftrong, that the coated glafs has frequently been lifted up by the brafs plate from the table; yet in a few days, being carefully placed in a proper repofitory (in contact with each other) not the leaft fign of electricity has been

(*h*) It has been fuppofed by fome gentlemen, that the very fame quantity of electricity imparted by the finger to the plate on touching it, was emitted again by the plate on removing it from the electric and prefenting it towards the knuckle; and that therefore, in air perfectly dry, this machine would at all times exhibit its phenomena without a frefh excitation of the electric, and thus merit the appellation of a machine for exhibiting perpetual electricity: but the fact above mentioned entirely refutes that fuppofition.

 difcoverable

difcoverable on their feparation: fo far is even this machine from exhibiting perpetual electricity. Indeed, in this particular, it is far exceeded by Mr. GREY'S apparatus of the cone of fulphur in the glafs, which, on being feparated, I have never perceived to fail of exhibiting ftrong figns of electricity, in every ftate of the weather. To this apparatus I have lately added the improvements of M. ÆPINAS, and find that they fully anfwer his report.

THE following paper contains a fet of experiments which may perhaps lead to fome curious, ufeful, and important truths in electricity. They are made with the moft fimple apparatus, and in the moft fimple manner; nothing more being requifite for this purpofe than a few fticks of fealing-wax (one of them being referved as a teft) to the ends of which the fubftances to be examined are to be fixed or tied as occafion may require, and Mr. CANTON'S electrometer, neatly made and properly infulated. With this fmall apparatus may almoft every article that can be propofed be examined with the utmoft facility. The animal, the vegetable, the foffil kingdom, with all the works and combinations of art and manufacture, may afford materials; almoft any of which, by a flight friction againft woollen cloth or filk, will become

electrified (either poſitively or negatively, according to the nature of the ſubſtance and form of its ſurface, and the quality and ſurface of the rubber) ſufficiently to ſeparate the balls of the electrometer, ſo as to determine their electricity in a very ſatisfactory manner. This, I think, fully confirms an opinion I have long entertained and mentioned in a former paper, *viz.* that the ſlighteſt friction of bodies of every kind, in every ſituation, may diſturb the electric matter contained in them, though the effect be imperceptible to us, having no electrometer nice enough to diſcover it. Here, therefore, is a boundleſs field for future enquiry; and, to aſſiſt thoſe who may be inclined to proſecute it, I ſhall mention a few general obſervations and precautions, which I have found exceedingly uſeful in the courſe of my own experiments. 1ſt, The air ſhould be dry, and the apparatus clean and warm. 2dly, The ſubſtances to be tried ſhould be perfectly clean. 3dly, When the rubber hath been uſed once or twice, it ſhould be held near the fire or the flame of a candle, not only to prevent its acquiring moiſture, but to take off the electricity left in it by one ſubſtance, before another be examined; for it ſhould always be remembered, that whenever a ſubſtance is made electrical by friction, the rubber acquires the contrary electricity, and this electricity, if it be not carefully taken off as is above directed,

will

will ſometimes remain in it ſo as to confuſe and actually miſlead in the experiments. 4thly, Some minute ſubſtances, as a ſmall leaf, ſeed, or hair, will not be eaſily excited in damp weather; theſe, therefore, as well as the apparatus, ſhould be warmed; for heat, I find, doth always diſpoſe bodies to become electrical. 5thly, The inſulating ſtands, and the ſticks of ſealing-wax, with which the ſubſtances to be examined are connected, ſhould not be rubbed, leſt they, by the friction, ſhould be made electrical, and, acting through the ſubſtance in contact with them, deceive in the experiment. 6thly, The animal ſubſtances, as hair, horn, bone, cartilage, nails, teeth, muſcles, &c. become electrified poſitively, by friction, againſt woollen cloth or black ſilk; and the vegetable creation, with very few exceptions, negatively. The metals differ with reſpect to kinds, form, and ſurface, and may be differently affected by different rubbers. Laſtly, I muſt not omit to obſerve that, among vegetables, I find the hot, acrid, pungent, and aromatic ſubſtances, as the ſpices, &c. to be much more eaſily excited, and ſtronger in their power, than the cold ones, as the ſeeds of gourd, melon, or cucumber. Among the herbs, hemlock and parſley are ſtrong: a ſingle leaf of laurel, bay, yew, roſemary, &c. will be found very powerful; but, as I have before obſerved, this field is indeed immenſe, and life itſelf too

ſhort

ſhort for a compleat inveſtigation. The experiments prove, however, how univerſally the electric matter is diſſeminated; or, in other words, its exiſtence in all bodies; with what readineſs it is excited; and, I think, the conſtancy of its action (though imperceptible to us) as well as its uſe and importance as a principal agent in the greateſt, and to mankind the moſt intereſting, operations in nature. I have encloſed a catalogue, exhibiting at one view the articles I have had an opportunity to try, with their kinds of electricity marked againſt them; hoping this ſpecimen may induce gentlemen of more leiſure to purſue the enquiry.

Hair, wool, down, and many other articles, may be made up in the form of little taſſels, and in this manner readily fixed upon or tied to the end of an electric for experiment.

The following ſubſtances being fixed or tied, upon the end of a ſtick of ſealing-wax, and excited by friction againſt a woollen garment or a piece of ſoft black ſilk, became electrified as marked in the columns of the annexed table. The ſtrongeſt in power are diſtinguiſhed by the letter s, and the weakeſt by the letter w.

METALS.

	Wool.	Silk.
A new guinea; a ſmooth ſix pence; a braſs ferule; tin, and tin-foil; enamelled copper, s; gilding on leather, s; lead ore; copper ore; iron ore; ſtream tin;	Neg.	Neg.
Milled lead; copper, s; a poliſhed ſteel button, s; a new ſilver ditto; a metal button gilt, s; tutenague ditto, s; iron;	Poſ.	Poſ.
Lead from a tea-cheſt, in which there is a mixture of tin, w;	Neg.	Poſ.
A gilt button, baſket pattern; the juncture at the end of a braſs ferule;	Poſ.	Neg.

ANIMAL

ANIMAL SUBSTANCES.

	Wool.	Silk.
Tortoiſe-ſhell, w; ivory, s; bone, s; horn; lamb's-tooth; horſe's-hoof; deer's-hoof; muſcle of the leg of a deer, s; cartilage, s; ſpur of a young cock; bill, claw, and ſcale from the leg of a turkey, s; ſcale of a carp; the *chryſalis* of a moth, recent from the earth, cleanſed; *craſſamentum* of the human blood exſiccated, w; quills; claw of an unboiled lobſter; cowrie and ſeveral other ſmooth ſhells, s; ſhell of a hen's egg; tail of a ſmall fiſh; thigh of the elephant beetle; a ſmall beetle, ſmooth ſurface; human hair; red and white horſe's and bullock's hair, s; hog's briſtles, s; wool; ſilk from the worm, w; oyſter-ſhell, ſmooth ſurface;	Poſ.	Poſ.
Mother of pearl, and ſeveral other ſhells;	Neg.	Poſ.
Muſcle and cockle-ſhells, recent; a recent ſnail-ſhell, rough ſurface; *elitra* of the ſtag-beetle; oyſter-ſhell, rough ſurface;	Neg.	Neg.

VEGETABLES.

	Wool.	Silk.
Rind of cheſtnut, s; Barcelona nut-ſhell, s; caſhew nut, s; cocoa nut-ſhell poliſhed; brazil; *lignum vitæ*; black ebony, s; box, w; cane, s; *quinquina*, or Peruvian bark, s; tamarind-ſtone; coffee-berry roaſted, s; nutmeg, s; ginger, s; white pepper, freed from the huſk, s; cinnamon, s; cloves, s; mace, s; all-ſpice, s; capſicum, both ſides of the pod, s; hemlock, s; a clove of garlic; ditto of eſchalot, freed from the huſk, s; a green onion, s; rue, s; cork, s; leaves of laurel, bay, yew, holly, roſemary, with their berries, s; parſley, s; leaf of turnip; ditto of Savoy cabbage, s; celery, s; ſago, s; thime, s; carrot; turnip; potatoe; an acorn, s; rind of Seville orange, s; a large Windſor bean, s; a white pea; root of the white lily; ſnow-drop root; ſeeds of gourd, melon, cucumber, w; a ſpecies of long-moſs, w; an apple, s; down of the cotton-ruſh, w; ſea-flag; leaf of the American aloe, s; cotton, w;	Neg.	Neg.

Hemp;

	Wool.	Silk.
Hemp; flax; ſtalk of the tobacco-leaf; ſpike, from the leaf of the American aloe; *palma-chriſti* nut; horſe-radiſh;	Neg.	Poſ.
A white kidney-bean, ſmooth ſurface; black negro of the ſame; ſcarlet of the ſame;	Poſ.	Poſ.
CORALLINES.		
Sea-fan, the horny part, w; rough coral, w;	Neg.	Poſ.
Spunge, w; coral poliſhed, w;	Poſ.	Poſ.
SALTS.		
Allum, w,	Neg.	Neg.
Borax, Nitre purified, ſmooth ſurfaces;	Poſ.	Poſ.
FOSSIL AND MINERAL SUBSTANCES.		
Common pebble-ſtones of all colours, s; marble, s; pit-coal, s; black-lead, w; jet, s; *aſbeſtos*; mineralized ſulphur; thunder-bolt ſtone; *cornu-ammonis*; ſhark's-tooth; coat of petrifaction;	Neg.	Neg.
Several ſmooth native cryſtals; brown Iceland ditto; *talc*, s; Ceylon pebble, ſmooth and tranſparent; agate, s; cornelian; amethyſt, s;	Poſ.	Poſ.

A ſpecimen

	Wool.	Silk.
A ſpecimen of *gypſum*,	Neg.	Poſ.

ARTIFICIAL SUBSTANCES.

	Wool.	Silk.
Staffordſhire ware glazed; China ware, s; Wedgwood's ware glazed, s; whale's fin prepared, w; writing-paper; parchment, s; ſheep's gut,	Poſ.	Poſ.
Tobacco-pipe, s; Wedgwood's ware unglazed; elaſtic gum, s; hard under-cruſt of a leaf; a tallow-candle, w; oiled ſilk; painted paper, s; ſilver, burnt into glaſs, unburniſhed; pearl-barley, w; Indian ink, w; blue vitriol, s,	Neg.	Neg.
Dr. LEWIS's glaſs porcelain,	Neg.	Poſ.

Silver burnt into glaſs, burniſhed, could not be excited either with the woollen or ſilk. It is ſuppoſed that the ſubſtances being ſo intimately incorporated or blended together, the friction at the ſame time excited both the ſubſtances, ſo as to counteract and deſtroy the effect of each other.

In theſe experiments I have been aſſiſted by Mr. CAVALLO and Mr. ADAMS, who have carefully repeated them in my preſence. It may be proper to obſerve alſo, that the white pea, a ſcarlet bean, and the thorn from

 the

the American aloe[b], being rubbed upon new, fine, claret-coloured cloth, became weakly electrified poſitively.

A lock of black hair from a young colt I have obſerved to become very weakly electrified negatively, when excited either with wool or ſilk.

It will be proper to obſerve, that the rubber of woollen cloth, which I uſed in the preceding experiments, was a part of the coat or waiſtcoat which I happened to wear at the time; but being deſirous to try the effect of another, I took a piece of fine, new, white flannel, and uſing that ſide of it which is free from knap, I perceived a remarkable difference; *viz.* the copper and other buttons, the different ſtones, ſhells, China ware, moſt of the animal ſubſtances (hair excepted) and all the vegetable ones, which with the former rubbers were poſitively, being now negatively electrical; but thoſe which were negative with them, I found to be negative with theſe alſo[i]. In all the experiments with the flannel, I had hitherto

(*b*) All theſe ſubſtances have ſmooth ſurfaces.

(*i*) On this account an ingenious friend of mine propoſed the following queſtion, *viz.* Whether, if the electric matter were inherent (as I aſſerted) in the different ſubſtances I examined, the kind of electricity could be changed by the uſe of different rubbers? I reminded him of Mr. CANTON's experiments with a glaſs tube, and informed him, that a ſtick of ſealing-wax becomes poſitively electrified by dipping it in quickſilver, or exciting it with a ſlip of tinfoil; that a ſmooth glaſs tube may be made negative by drawing it croſs-wiſe over

hitherto warmed the ſubſtances, but kept the rubber cold; ſuſpecting that, in ſome caſes, the reſult might be different from what it is when the rubber is warm and the ſubſtance cold: but this, with the effect of cloths of different textures and colours, ſilks, ſattins, velvets, leather dreſſed in oil, and in allum, &c. uſed as rubbers, I have not had leiſure to determine. A ſmall turnip and a potatoe, which I could not excite at all with either of the rubbers when they and the ſubſtances were cold, I excited in a very ſmall degree with the flannel a little warmed; but very ſtrongly when the flannel and the reſpective articles were, each of them, moderately warm. A ſprig of celery acted very powerfully when the flannel only had been previouſly warmed (k). As in all caſes the rubber is affected with an electricity contrary to that of the ſubſtance rubbed, it will be ſufficient for

over the back of a cat, or by exciting it with a dry, warm rabbit's ſkin; that a ſmall coated bottle may be charged with a ſlip of writing-paper, excited by drawing it briſkly between the fingers of a dry hand ſo as to pierce a hole in a card; that the dry leathern cover of a book may be made ſtrongly electrical by the friction of a dry hand, and that its electricity is remarkably ſhewn by touching it with an inſulated button, in the form of the plate to Mr. VOLTA's machine. He then acknowledged, that ſuch an objection as he had ſtarted muſt certainly be groundleſs.

(*k*) A *palma-chriſti* nut was excited very ſtrongly with the flannel, weakly with my coat, and not all (in a room where there was no fire) with the black ſilk. I have alſo to add, that ſome particular ſubſtances, though negatively electrical when heated, become poſitive when cold, by friction againſt the very ſame rubber.

many purpoſes to uſe a rubber with a handle of glaſs, wax, &c. with which the electricity of many fixed bodies, gems that are ſet in metals, &c. and other articles that it would be improper to divide or remove, may be determined. Such rubbers of different forms have been conſtructed and ſatisfactorily employed in a variety of inſtances by Mr. ERRINGTON and Mr. CAVALLO, who have extended their enquiries far beyond the limits at which I deſiſted; their collection of animal, vegetable, foſſil, and artificial ſubſtances, added to my catalogue, amounting to almoſt one thouſand articles.

C O N C L U S I O N.

AT the concluſion of the ſecond part of the preceding paper, in my remarks upon the electricity of chocolate, I have obſerved, that many and great diſcoveries have been made in this age, reſpecting the action, influence, and effects of electricity; but it is a queſtion that hath been frequently put to electricians, What is electricity? For my own part, I have generally choſen (perhaps for want of a better anſwer) to reply by a ſimilar queſtion, *viz.* What is air? or, what is water? For, as theſe are underſtood to be fluids diſtinct from all others, and

 diſtinguiſhed

diftinguifhed by the names they bear, fo have I ever confidered electricity as a fluid *fui generis*, and properly characterifed by the term electricity, electric fluid, or electric matter; and have always avoided the term electric fire, as conveying a confufed idea of actual inflammation, burning, &c.: but I now begin really to doubt, whether another appellation might not be applied with greater propriety; whether electricity may not be confidered as a pure, ethereal, elementary fire, inherent in all bodies, intimately connected or blended with an earthy or other bafe, and apparently, though not actually, remaining in it in a quiefcent ftate, till roufed into action by fome proper application, as motion, or rather friction, which may, and probably does, collect it in our experiments. (But can motion convey inftantaneoufly that which is not material, but only a quality, a property, an accident, or affection, of matter, through fuch circuits as thofe of Dr. WATSON, and produce fuch aftonifhing effects at the interruption of thofe circuits? Befides, in Dr. FRANKLIN's moft curious and decifive experiment of charging the Leyden bottle with its own electricity, the glafs undergoes no friction whatfoever; but the electricity inherent in it is fimply exhaufted from one of its furfaces, and forced round upon the other by the electrical apparatus: the fame may be afferted of bodies prefented toward a conductor negatively

 electrified,

electrified, or to the inſulated rubber of the electrical machine.) That it may be ſaid to reſide in vegetables, and is extracted together with their oil; that in fermentation, effervefcence, and putrefaction, it flies off in the phlogiſtic vapour thence ariſing (ſee note 1. at the concluſion of this paper); that in diſtillation it is diſengaged and brought over in an ardent ſpirit, in which it reſides, retaining its original properties in a purer baſe; that, ſince by the colliſion of flint, ſteel, &c. actual fire is inſtantaneouſly produced (as in the inſtance of the dry axle of a carriage, which, by the friction of the nave againſt it, ſoon takes fire) ſo by the friction of other bodies, which by long perſeverance would produce the ſame effect, this latent fire may be firſt excited, and its appearances, though unobſerved, be thoſe we term electrical. A wind-mill, when it works under the break (as the millers term it when no iron is concerned) ſoon catches fire (the mill-ſtones, when no corn is between them, produce the ſame effect, though the motion be the ſame in both caſes) and many a mill hath been conſumed by this means. The method uſed by the Indians, of producing fire by the friction of two pieces of wood againſt each other is well known; and in all theſe caſes may not the firſt effects of the latent fire, thus rouſed into action, be the production of thoſe very appearances we call electrical? See notes 2. 3. 4. 5. and 6.

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This thought, I confefs, remained fo ftrongly impreffed upon my mind, that I requefted fome of my friends, who had a better opportunity than myfelf, to make the trial. For this purpofe fome pieces of wood were baked in an oven, in order to expel the moifture, and prepare them for the experiment. When they were cooled, a friction was begun, which, as I expected, foon produced electricity; one piece of the wood being excited pofitively, the other negatively, as I have fince myfelf feveral times experienced. Had the friction been continued, the production of actual fire might perhaps have been the confequence. May not, therefore, the production of actual fire be the *ultimum* of electricity? or, in other words, electricity the firft effect of latent fire thus roufed into action; actual fire, the fecond; and inflammation and diffolution, its third and greateft effort? like fermentation, producing firft, wine; fecondly, vinegar; laftly, putrefaction. To give fome countenance to this fuppofition, let fome of the effects of electricity and fire be placed in a comparative view. Firft, a fmall iron wire, held in the flame of a candle till it acquires a white heat, will frequently burft into little balls, flying off in all directions. The fame effect is produced by a flint and fteel; and in a fuperior manner, by a ftrong charge of electricity, or a flafh of lightning paffing through fuch

a ſmall wire; the balls then appearing, on examination, to be little more than the *ſcoriæ* of the metal. The effect of electricity, lightning, and fire, in deſtroying the power of the artificial or natural magnets, is a circumſtance that hath been often remarked, and repeatedly publiſhed. The effects of electricity, in common with fire, on proof-ſpirit, gun-powder, *phoſphorus*, dry lint, and many other ſubſtances, muſt occur to every gentleman converſant in theſe experiments; indeed the parallel might be continued much further. But it may be aſked, if this be really the fact, ſhould not metals become electrical by friction? I anſwer, they are readily excited, provided they be firſt properly inſulated; (but if metal be rubbed againſt metal, the phlogiſton or latent fire, if I may be allowed the expreſſion, is ſo nearly proportioned in the two metals, that the equilibrium is reſtored as ſoon as deſtroyed, from the very nature of the baſe, which is the moſt perfect conductor we are acquainted with) to illuſtrate this, let it be remembered, that though the hydroſtatic paradox may be readily explained, yet the fluid muſt be confined in a proper veſſel; and though the weight, the ſpring, and the compreſſibility of the air, be eaſily demonſtrable, a ſuitable apparatus muſt neceſſarily be employed for each purpoſe.

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It is a queſtion by no means decided, how the clouds become electrified? But if we ſuppoſe the electric matter to be a pure, ethereal, elementary fire, reſident in all bodies; that the great proceſs of vegetation is carried on by means of this ſubtile, active, volatile, and pervading element; that it is continually exhaling from all the vegetable tribe; that as evaporation is a remarkable agent in the cooling of heated ſubſtances, that is, a good conductor of their fire, as I am well aſſured it is of electricity; may we not conclude, that this is one great cauſe of the clouds becoming at times ſurcharged with this fluid? The great effect of electricity in promoting vegetation, hath been fully proved by Dr. DE MAIMBRAY, the abbé NOLLET, Mr. JALLABERT, and other gentlemen, and was very remarkable in that year when the fatal earthquake happened at Liſbon. Dr. STUKELEY's obſervations on the frequent appearances of fire-balls, coruſcations, and *auroræ boreales*, at this time (which I well remember) deſerve to be particularly noticed; and it is generally remarked, that thunder-ſtorms are preceded by a continuance of hot weather, and that a moderate temperature immediately ſucceeds the ſtorm. The remarks and obſervations of the worthy Dr. HALES on this ſubject ſeem alſo to merit peculiar attention. Further, as the rays of the ſun, concentrated by a powerful burn-

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ing mirror, will produce a fufion of metals, and inftantly reduce a number of fubftances prefented to the focus to a *calx*, as the fame effect is in many cafes produced by a ftroke of lightning; and as the colours of the electric and folar light are equally divifible by the prifm; may not thefe alfo bear fome kind of relation to each other[l]? Upon the whole, is there not an high degree of probability in the fuppofition, that light, fire, phlogifton, and electricity, are only different modifications of one and the fame principle? See notes 7. 8. and 9. A fimilarity in feveral of the phenomena of electricity and magnetifm hath been long fince pointed out by Dr. PRICE, from M. ÆPINAS; and the effect of heat on both admirably difplayed by Mr. CANTON. Of all the fubftances I have yet examined, the moft difficult to excite, I obferved to be a fine, fmooth, unarmed load-ftone, and a piece of black lead; thefe feemed to bid defiance to all my rubbers: at length, however, with a piece of new flannel they were both excited, in a very fmall degree, negatively. In fhort, I have not yet met with a fingle article (on which the experiment could be tried) that I could not, with one or other of my rubbers, make in

(l) Many other particulars might be adduced in this place; but they are purpofely omitted, this paper being already extended far beyond the limits originally intended by the author.

fome

ſome degree electrical. The laws by which all theſe fluids are governed, and what conſtitutes the preciſe difference between them, may yet, perhaps, by ſome fortunate philoſopher, by a train of juſt reflexion, and a ſet of happily contrived and well-conducted experiments, be much farther elucidated. Laſtly, I do not ſpeak of theſe things as facts of which I am abſolutely convinced; but earneſtly wiſh to recommend them to the ſerious conſideration of future enquirers. From what hath been ſaid, however, I apprehend it will ſcarcely be doubted, that electricity, whatever it be (as I have often remarked) is one of the greateſt and moſt important agents in the operations of Nature; that the effects of lightning, therefore, are but as diſcords in her harmony; and, though ſingly conſidered, they may appear unpleaſing notes, yet perhaps may be neceſſary to fill up and compleat her grand and general chorus.

NOTES ON THE CONCLUSION.

1. I am juſt informed by Mr. ADAMS, that Mr. CLARKE, an ingenious gentleman from Ireland, hath lately proved, to the ſatisfaction of ſome of the ableſt chemiſts there, that the variety of airs produced by different gentlemen

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in their pneumatical reſearches (that produced from the *calces* of metals perhaps excepted) are only phlogiſtic vapours ariſing from, and partaking of, the qualities of the ſubſtances from which they are diſengaged.

" The vapour of fermentation is much more ſubtle " than common air, it paſſes through bodies which " would be impenetrable obſtacles to the latter.

" Mr. DE SMETH was not able to retain it by the aid " of lutes: a moiſtened bladder, tied over the mouth of a " veſſel which contained ſome fermenting matter, was " not at all inflated during the height of fermentation.

" Dr. PRIESTLEY has obſerved, that the fixed air from " fermenting beer combines eaſily with the vapour of " water, as alſo with the ſmoke of roſin, ſulphur, and " other electrical ſubſtances.

" If it were permitted me to indulge in conjectures, I " ſhould ſay that ſome experiments induce me to be- " lieve, that every elaſtic fluid reſults from the combina- " tion of ſome ſolid or fluid body with the inflammable " principle, or perhaps even with the matter of pure " fire; and that on this combination the ſtate of elaſti- " city depends." See HENRY's tranſlation of M. LAVOISIER's Eſſays, phyſical and chemical.

Mr. LANE, in his curious and moſt important experiment of diſſolving iron in water impregnated with fixed

 air,

air, obſerved, that after the water ſo impregnated had been paſſed through a cloſe filtering paper, it was rendered quite tranſparent, the iron being in perfect ſolution. This clear liquor he endeavoured to preſerve in its tranſparent ſtate, by uſing every means that then occurred to him to retain the elaſtic vapour, but without ſucceſs, for in a few hours the tranſparency diminiſhed: afterwards the liquor became opaque, and depoſited the iron that had been diſſolved in it.

2. Several gentlemen have obſerved, that in working their electrical machines with great velocity, as heat was produced by the friction, the electricity was proportionably leſſened.

3. Mr. ERRINGTON, a gentleman who often recreates himſelf with mechanical operations, frequently obſerved, that after he had been for ſome time briſkly working his drill, the ſtring of it became ſtrongly electrical.

4. Mr. CAVALLO, who ſometimes amuſes himſelf with the violin, having played a few ſprightly airs, examined at my requeſt the hairs of the bow, and the ſtrings of the inſtrument, and found by his electrometer that both of them were electrical; the former in a *plus*, the latter in a *minus* ſtate. In this caſe, the roſin contributes to

the electricity. Perhaps a tenor or baſs-viol might produce the effect in a greater degree.

5. Mr. CAVALLO likewiſe informs me, that taking two pieces of broken China ware, he ſtruck the edges of them briſkly together, and produced ſparks of fire, but no electricity. He then rubbed the broader ſurfaces gently together, and produced a ſtrong electricity; poſitive in one piece, negative in the other. This experiment I have ſeveral times repeated to my entire ſatiſfaction.

6. I have myſelf obſerved, that two glaſs tubes, rubbed briſkly together, produce a vivid purple light and ſtrong phoſphoreal ſmell, but no attraction or repulſion; but two pieces of plate glaſs, each two inches long and one inch broad, warmed and rubbed gently againſt each other, produce electricity, negative in one piece, poſitive in the other. Both glaſs and amber I have alſo made electrical by blowing upon them (previouſly warmed) with a pair of bellows.

7. Platina, in the pureſt ſtate to which it could be reduced by chemiſtry, and on which Dr. LEWIS informed me that the ſtrongeſt fires he could raiſe had no further effect, I have been able to fuſe in a ſmall degree, by a ſtrong charge of electricity. Phil. Tranſ. vol. LXIV. p. 416.

8. Since the learned and accurate F. BECCARIA published the account of his curious experiment of revivifying the *calces* of metals by electricity, it hath been repeated with perfect ſucceſs by ſeveral other gentlemen.

9. With reſpect to earthquakes, upon this hypotheſis Dr. STUKELEY's and the rev. Mr. MITCHEL's ingenious theory may both be near to truth, as the difference between them will conſiſt more in words than in facts. See a moſt curious and aſtoniſhing effect of evaporation produced by electricity in Dr. FRANKLIN's Experiments and Obſervations, firſt edit. p. 415. Perhaps it may not be improper to mention in this place the following experiment, which I made long ſince myſelf. A pretty large wine-glaſs being nearly filled with water, two wires, terminated by ſmall braſs balls, were hung oppoſite to each other upon the brim of the glaſs, ſo as to let the balls deſcend to about half the depth of the water. The communication being then compleated by a chain, a jar containing three ſquare feet of coated ſurface, was diſcharged through it. The conſequence was, the ſtem of the glaſs was broken in two places; the bowl was ſhivered perhaps into a thouſand pieces, and ſcattered with the water in all directions: part of it flew into my face, and ſo much upon the apparatus, that I remember it put an end to my experiments for that time. I had neglected to cover the glaſs, being deſirous to ſee the

effect

effect of the charge paſſing through the water; not ſuſpecting the danger of the electricity evaporating part of it, and exploding with ſuch violence as might have been attended with very diſagreeable conſequences.

10. That water-ſpouts are really occaſioned by electricity I have long ſuſpected, from ſeveral circumſtances; but Mr. GEORGE FORSTER, F. R. S. in his curious remarks and circumſtantial deſcription of one of theſe phenomena (Voyage round the World, vol. I. p. 191.) ſeems to have confirmed this matter beyond a doubt: the form of the column, the hail-ſtones which fell at the time, and the flaſh of lightning which appeared at the disjunction of the tube, are, I apprehend, as complete proofs as can be given, or as the caſe can admit or require.

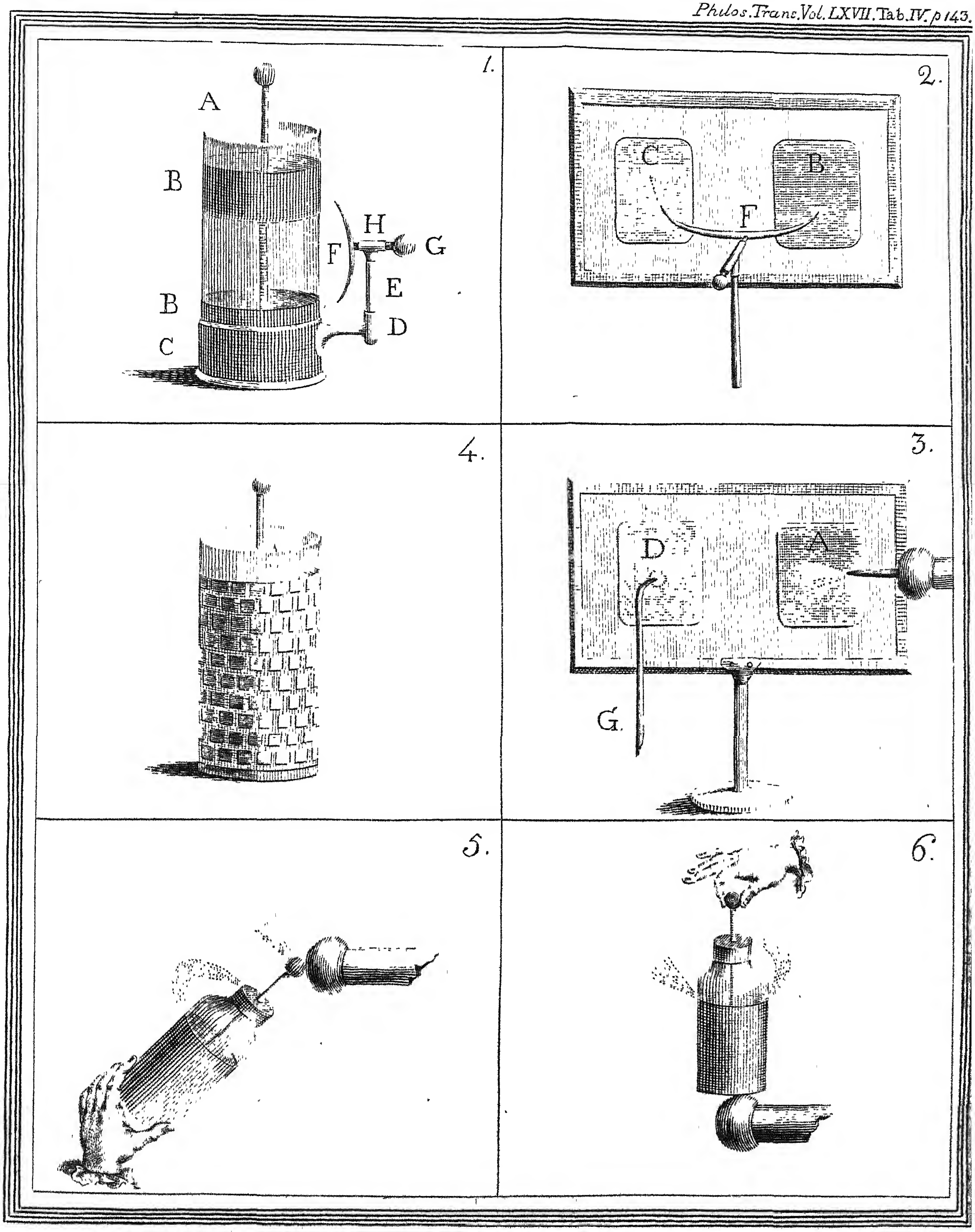
11. Some gentlemen have ſuppoſed, that the electric matter is the cauſe of the coheſion of the particles of bodies. If the electric matter be as I ſuſpect, and my experiments and the foregoing notes ſeem to prove, a real elementary fire inherent in all bodies, that opinion may probably be well-founded; and perhaps the ſoldering of metals and the cementation of iron by fire may be conſidered as ſtrong proofs of the truth of their hypotheſis.

12. Dr. PRIESTLEY obſerves (Experiments and Obſervations on Air, vol. I. p. 280.) That it is probable, that electric

tric light comes from the electric matter itſelf; that this being a modification of phlogiſton, it is probable that *all light* is a modification of phlogiſton alſo; and that, prior to his deductions from electrical phenomena, it was pretty evident that light and phlogiſton are the ſame thing in different forms or ſtates. Dr. PRIESTLEY's third volume on the ſame ſubject was not publiſhed till the laſt ſheet of my paper had been compoſed, and a proof ſtruck off. In the appendix to that volume I find ſo curious an article in a letter from ſignor VOLTA to the Doctor, that I ſhall take the liberty to tranſcribe a part of it, as a very important addition; *viz.* "I fire inflammable air "by the ſimple electric ſpark, even when the electricity "is very moderate, which explains the *ignes fatui*, pro-"vided they conſiſt of inflammable air iſſuing from "marſhy ground by the help of the electricity of fogs "and by falling-ſtars, which are very probably thought "to have an electrical origin.

"I do not know whether you have ever tried the effect "of the Bolognian phoſphorus on air. It phlogiſticates it "in the higheſt degree, and the diminution it occaſions "takes place very quickly, and is altogether ſurpriſing; "but for this purpoſe the phoſphorus muſt be good, "and the weather not too cold."

Since



Basire sc.

Since the preceding papers were preſented to the Royal Society, I have ſeen and heard of ſuch a number of curious remarks, obſervations, and diſcoveries on light, fire, phlogiſton, and electricity, which tend to illuſtrate and confirm the opinions I have advanced, that I would beg leave to add, that, had I ſeen or known of ſeveral of thoſe excellent pieces in time, I ſhould certainly have availed myſelf of ſuch important labours, and have ſpoken of the ſubjects above-mentioned with a greater degree of confidence. The authors I allude to are, BOERHAAVE on Fire; STAHL on Phlogiſton; Dr. PEMBERTON on Fire; Dr. HIGGINS on Light; the celebrated MACQUER, particularly in his Memoir on Phlogiſton, in the abbé ROZIER's Journal for Nov. 1776; a Memoir by Mr. OPOIX on Phlogiſton and Light; Mr. ACHARD's Electrical Experiments on the Ice of diſtilled Water, frozen in a degree of cold exceeding what we ever experience in this country; with plates of which ice he not only performed the Leyden experiment, but even excited it by friction like glaſs (ſee the Abbé ROZIER's Journal for Nov. 1776); and laſtly, M. KOESTLIN's curious and moſt valuable experiments on the influence of electricity, in the production and ſupport of animal and vegetable life, particularly his diſcovery that vegetation was actually retarded by electrifying his ſeeds negatively

VIII. *Extract of a Letter from* John Strange, *Esquire, His Majesty's Resident at* Venice, *to Sir* John Pringle, *Bart. P. R. S.: with a Letter to Mr.* Strange *from the Abbé* Joseph Toaldo, *Professor in the University of* Padua, &c. *giving an Account of the Tides in the Adriatic.*

Venice,
Nov. 19, 1776.

S I R,

Read Jan. 23. and Feb. 6. 1777.

I N your last favour you expressed a desire of having some account of the the course of the tides here. As I have hitherto had but little opportunity of making any conclusive observations on that subject, I applied to the Abbé TOALDO, professor of astronomy and meteorology in the university of Padua, as the most likely person to inform me. He obligingly complied with my request by the enclosed letter, which I hope will be acceptable to you, and in some measure satisfactory, being chiefly grounded on the observations of a very accurate man here, signor TEMANZA, a celebrated architect and engineer. It is a pity but those observations had been extended to a longer term; but as no better are to be had at present, we must be contented, and hope for better hereafter. In the mean time it will be a further satisfaction to the learned professor, as well

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as to me, SIR, ſhould this communication prove alſo acceptable to the gentlemen of the Royal Society, to whom you may probably think proper to preſent it.

De reciproco Æſtu Maris Veneti. Ad Nobilem ac Doctiſſimum *Joannem Strange*, &c. Epiſtola *Joſephi Toaldi*, &c.

QUÆRENTI tibi, vir præſtantiſſime, notitiam reciproci maris æſtûs, in portu atque æſtuario Veneto obtinentis, idque nomine Regiæ Societatis veſtræ, clariſſimique ejus præſidis D. JOHANNIS PRINGLE, baronetti, libentiſſime, pro modo ac viribus, morem gero. Quî enim negare poſſem vel tibi, ſtudiorum meorum fautori munifico, rerumque naturalium ſcrutatori eximio? vel Regiæ Societati, cujus acta cognitionum humanarum rerum gazophylacium, in publicum uſum paratum, exhibent? vel illuſtri ejus præſidi, ob ſcientiarum amorem, in Europâ totâ tam celebrato? vel ipſi heroicæ nationi Britannicæ, pro rerum maritimarum præſtantiâ, notitiam hanc præcipuo jure quaſi petenti. Accedit, quod res hæc illuſtrium autorum falſâ traditione infecta ac turbata eſt, ſicut mox docebo; ut proinde interſit non minus navigationis quam ſcientiæ, veram ejus hiſtoriam

 memoriæ

memoriæ ac litteris tradere. Conabor ergo præcipua ac ſumma rei capita breviter colligere. Ipſe quidem, Patavii degens, obſervationum ſeriem, quod utinam licuiſſet, inſtituere non potui. Sed D. THOMAS TEMANZA, architectus Venetus clari nominis, diaria obſervationum ſuarum liberaliter mecum jamdiu communicavit; ac, licet ſemel in die obſervaverit, aquarum tamen motus ac leges in æſtubus reciprocis ſatis indicant: et autores alios conſului ſatis idoneos; et ipſe identidem, Venetias digreſſus, adnotare aliqua minime neglexi.

1. Primum igitur, illud etiam vulgo notum, Venetiis, nempe in portu atque æſtuario Veneto, ſingulis diebus, bis aquarum acceſſum ſive fluxum fieri, biſque receſſum, ſive refluxum; ſingulis vero menſibus lunaribus acceſſus fieri majores per aliquot dies circa novam plenamque lunam, quæ Veneti appellant *Punti d' acqua*, quibus tantum, navigia majora in portum compelli poſſunt, vel ab eo educi.

2. Quod præcipue ſcire intereſt in hoc negotio eſt hora, quâ æſtus menſtruus ad culmen pervenit, quâque portum pertranſire poſſunt. Dubitare quis poſſet propter JANI PLANCI, aliorumque fortaſſe ſcriptorum autoritatem. PLANCUS autem in *Specimine reciproci æſtûs in mari ſupero*, recuſo Romæ A. D. 1760, una cum libro *de conchis minus notis*, haud hæſitanter tradit, æſtum

æſtum maris ſequi curſum potius ſolis quam Lunæ; id eſt, aquas in quolibet acceſſu maxime elatas deprehendi ſole meridianum obtinente, maxime vero depreſſas ſole ad horizontem delato. Quid PLANCUM alioſque in errorem induxerit, pronunciare non auſim. Fortaſſe cauſa erroris fuit hæc: æſtus majores contingunt circa novam plenamque Lunam: iſtorum autem acceſſuum hora parum diſtat, ut mox oſtendam, a Lunæ tranſitu per meridianum; qui iis diebus parum item diſtat a tranſitu ipſius ſolis per eundem circulum. Ecce ergo cauſam erroris, quæ certè impoſuit ipſi JANO PLANCO; qui, ut in præfatione libri ſui fatetur, ad portum Ariminenſem, et ad motum aquarum obſervandum, non niſi tempore majorum æſtuum novilunii ac plenilunii deſcendere conſueverat; circa quadraturas, æſtus modici ſunt, vixque ſenſibiles, ut proinde ipſorum horam (turbatam etiam ob divulſionem ſolis ac Lunæ) obſervare non tam ſit promptum.

3. Quæcumque fuerit errandi cauſa certe errarunt: æſtus enim maris, ut alibi, ita Venetiis, regitur a motu Lunæ. Doctor VINCENTIUS MIOTTI, obſervator diligens ac navus, MURIANI, quæ urbs milliari uno circiter a Venetiis diſtat ſeptentrionem verſus, anno 1766 et 67, cum tabulas quaſdam huc pertinentes conficere vellet, per plures menſes æſtus accedentis ac pleni horam exploravit, eamque, in ſummâ, comperit eſſe unam cum di-

midiâ ante tranſitum Lunæ per meridianum ſuper vel ſubter; vel potius decimam cum dimidiâ poſt: dico potius poſt, quia credibile eſt, aquam, quæ ob æſtum in Mediterraneum ex oceano influit, tantum temporis inſumere ut ad extremas oras maris Adriatici perveniat. Eandem horam colligo ex obſervationibus anno 1770 hortatu meo habitis a laudato domino TEMANZA (nam in diariis præcedentibus hoc neglexerat): idem ipſe, cum Venetiis non ſemel eſſem, ſtudioſe rem animadvertens atque explorans, verum eſſe deprehendi.

4. Hora ergo completi portûs, quem ſtatum *etabliſſement du port* appellant Galli, id eſt æſtûs altiſſimi in ſyzigiis præcipue, eſt $10\frac{1}{12}$ poſt Lunæ tranſitum per meridianum, ſuperiorem, aut inferiorem, ita ut poſtea deſcendere incipiant. Lunâ vero ad horizontem, ſive orientalem ſive occidentalem, accedente, aquæ humiles ac depreſſæ ſunt, ac tum demum creſcere incipiunt[(a)].

Ex.

(a) Hujuſmodi aquarum vices in portu Veneto atque æſtuario, ſi autoritate opus ſit, traditas deprehendo in veteri codice manuſcripto, rationem univerſam Venetæ navigationis, tunc temporis continente, una cum ampliſſimo portulano: conſulendum olim præbuit ſereniſſimus MARCUS FOSCARENUS, Venetiarum prius hiſtoricus, deinde dux, vir dum viveret artium ac doctorum patronus munificentiſſimus; ſcriptus eſt codex linguâ Venetâ vernaculâ, eâque antiquiore. Opinabatur ſereniſſimus dux, fuiſſe codicem PETRI LAURETANI, ſtrenuiſſimi ducis maritimi ex noſtris, qui anno 1443 extrema nece Januenſes apud RAPALLUA debellavit. Is ergo codex inter præcepta navigandi ad rem noſtram hæc habet, p. 51. *Le acque di queſto porto* (di Vinetia) *ſi xè queſte: quando la Luna*

xè

Ex hoc igitur ſcriptore (in adnotatione) habemus vices æſtûs, cum diurni tum menſtrui, in portu Veneto. Quod vero aſſerit, Lunâ in meridiano exiſtente aquas fieri plenas, latius eſt ſumendum; ut intelligatur circa illam horam, id eſt cum parvâ differentiâ unius ac dimidiæ horæ, quam ſuperius ſtatuimus.

5. Conſequitur, ut dicamus de magnitudine, ſive altitudine aquarum in æſtu. Circa ſyzygias creſcunt aquæ plerumque tres pedes, vel tres cum dimidio fere, juxta menſuram Venetam (pes autem Venetus ad Londinen-

xè in Ponente e Levante, ſono tutte le acque baſſe; e quando xè in Siroco e Maiſtro, ſon mezze piene; e quando la Luna xè in Oſtro, le acque ſon tutte piene; e quando la Luna xè in Greco e Garbin, le acque ſon mezze vode.

Aviſote, che le acque in ſede comenza zorni quattro della Luna infin a zorni 10; *e la xè a ponta; e dai* 11 *inſuſo ſono acque ſeconde infina a zorni* 19 *della Luna; e dai zorni* 19 *infina zorni* 25 *ſono le acque in ſede; e dai* 25 *infina quattro della Luna ſono creſcenti. Aviſote, che in una Luna ſono do ſede, e do ſeconde; e da* 7 *alli* 9 *l'acqua no xè move, zoè non à poſſo:* quæ hanc vim habent.

" Regula aquarum in portu Veneto eſt hæc: cum Luna eſt in occaſu vel " ortu, aquæ ſunt prorſus humiles; cum reſpicit plagas intermedias ventorum, " id eſt cum mediam altitudinem aſcendendo vel deſcendendo occupat, aquæ ſunt " ſemi-plenæ, vel ſemi-vacuæ. Denique cum Luna eſt in auſtro, vel meri- " diano, aquæ ſunt ubique plenæ."

Hæc de æſtu diurno, quod ſequitur pertinet ad æſtus menſtruos.

" Admoneo, aquas deficere incipere, vel æſtus parvos fieri a quartâ die Lunæ " uſque ad decimam, ac tum incipiunt creſcere; a die undecima uſque ad unde- " viceſimam ſecundæ ſunt, vel plenæ; a decimâ nonâ ad viceſimam quintam " rurſus deficiunt, et deinde iterum creſcunt uſque ad quartam diem Lunæ. " Admoneo intra Lunationem eſſe duas periodos, minorum, totidemque ma- " jorum æſtuum; a ſeptimâ autem die ad nonam (addendum etiam 22 ad 24) " aquas minime moveri."

ſem ſeſe habet ut 1540 ad $1351\frac{2}{3}$) raro ad quatuor, rarius ad quinque, rariſſime aut fere numquam ad ſex, auſtro flante, mari irato ac procelloſo.

6. Circa quadraturas elatio aquarum multo minor eſt, aliquando vix trium pollicum; media, ut poſtea oſtendam, eſt pedis $1\frac{1}{3}$.

7. Indicata altitudo æſtûs, maxima eſt quæ obſervetur in toto Adriatico (cumulantur enim aquæ intra Veneta æſtuaria): quo enim magis acceditur ad Mediterraneum, minores continuo deprehenduntur æſtus, ita ut in Mediterraneo ipſo vix animadvertantur, exceptis euripis, aliisve anguſtis ſinubus.

8. Omitto quæ ſunt communia æſtûs maritimi in omnibus maribus; ex. gr. retardare ſingulis diebus cum Lunæ tranſitu per meridianum; ex duobus diurnis æſtubus, alterum eſſe majorem et diuturniorem; æſtus maximos circa ſyzygias fere nunquam contingere ipſo die ſyzigiæ, ſed vel antevertere, vel retardare, uno, duobus, tribus, aliquando vel quatuor diebus.

9. Peculiaria quædam adnotabo, ut illud cum JANO PLANCO, aquas, initio acceſſûs, paulatim ac lenè creſcere per tres aut quatuor horas, ut vix tres pollices horis ſingulis aſcendant; poſtea vehementer inſurgere curſu valde citato; tum fieri, in culmine, quoddam quaſi *aquiſtitium*, ut ſemihorâ quieſcere videantur, poſtea velociter deſcendere,

descendere, et easdem vices reciprocè subire; unde patet spatium transeundi portûs quatuor horis ad summum in majoribus æstubus circumscribi.

10. Illud præcipue peculiare videtur huic mari nostro, atque Adriatico toti, æstus æquinoctiorum (Lunâ novâ vel plenâ) esse quidem magnos, sed non totius anni maximos; sed maximos contingere circa solstitium hybernum. Sciendum præterea, sinum Venetum pleniorem in genere deprehendi, et aquas altiores esse hyeme, quam æstate: animadverterat hoc JANUS ipse PLANCUS in laudato specimine. Id ostendo duplici comparatione.

11. D. TEMANZA observationes suas instituit ab anno 1751 ad 1755. Ex hoc quinquennali diario excerpsi æstus medios singulorum anni mensium, quos in duas classes tribuo, sex hybernos, sexque æstivos.

Menses hyberni.	Æstus med. in ped.	poll.	Menses æstivi.	Æstus med. in ped.	poll.
Januarius,	2	1,9	Aprilis,	1	9,9
Februarius,	2	0,3	Maius,	1	9,5
Martius,	1	9,7	Junius,	1	11,7
October,	1	10,9	Julius,	1	9,9
November,	2	1,4	Augustus,	1	7,9
December,	2	2,6	September,	1	9,2
Med. diurnus,	2	0,5	Med. diurnus,	1	9,7

Patet

Patet ex hâc tabellâ, 1°, Maximum æſtum totius anni, Venetiis, contingere menſe Decembri, ſcilicet circa ſolſtitium hybernum; et huic proximum eſſe æſtum menſis Januarii. 2°, Minimum accidere menſe Auguſto. 3°, Mediocrem eſſe menſibus æquinoctialibus Martio ac Septembri. 4°, Inter ſex menſes æſtivos, menſem ſolſtitialem Junium afferre præ cæteris æſtum magnum, et quidem majorem æquinoctialibus ipſis. 5°, Tandem menſuram æſtuum æſtivorum valde ſuperari a menſura æſtuum hybernorum.

12. D. TEMANZA, in æſtu adnotando, reſpexit ad terminum quendam medium, qui Venetiis ab aquarum architectis appellatur *commune:* intelligunt libellam quandam mediam aquarum in lacunâ. Quoad ergo hanc libellam, ſive altitudinem mediam aquarum, adnotavit, quæ menſura æſtûs, tam accedendo quam recedendo, fuiſſet ſingulis diebus ſupra vel ſubter (accidit enim aliquando ut æſtus ad eam altitudinem mediam minime pertingat). Contuli ego in ſummas ex unâ parte dies, quibus fuit ſupra, ex aliâ, quibus fuit infra; idque pro utrâque menſium claſſe.

Æſtus menſibus hyb.	Dies ſupra med.	Dies inf. med.	Æſtus menſibus æſt.	Dies ſupra med.	Dies inf. med.
Januario,	105	51	Aprili,	82	68
Februario,	86	55	Maio,	85	80
Martio,	95	60	Junio,	73	77
Octobri,	86	69	Julio,	64	91
Novembri,	98	52	Auguſto,	63	92
Decembri,	102	53	Septembri,	75	75
Summæ,	572	340		442	483

Etiam ex hâc tabellâ diſcimus, æſtus hybernis menſibus fieri ſæpius elatiores, et ſinum noſtrum pleniorem eſſe, quam æſtate.

13. Phænomenon hoc ab aliâ cauſâ repetere neſcio, quam a viciniâ majore ſolis, in fine Decembris ad perigæum delapſi; niſi forte partem aliquam ſibi vindicet incitatio major, eo temporis, in annuo telluris motu; ut aliquid fortaſſe concedendum ſit etiam ingenioſæ GALILÆI theoriæ; ut dum aquæ, vi attrahente corporum cœleſtium, attolluntur, patiantur ſimul agitationem aliquam ob inæqualem motum telluris.

 14. Porro

14. Porro ſex illis menſibus hybernis contingunt etiam procellæ majores, atque inundationes urbis, in viis, ſoris, ac locis minus eminentibus. Hoc loco quæſtionem de majore altitudine aquarum, atque elatâ ſuperficie maris noſtri minime movebo. Illam tractarunt abunde CL. MANFREDIUS in *Actis Academiæ Bononenſis*, tom. II.; JANUS PLANCUS, l. c.; CL. ALBERTUS FORTIS in *Deſcriptione inſulæ* CHERSO ATQUE OSERO. § XVI.; probaruntque, aquas Adriatici reverâ altiores eſſe hiſce annis quam ſæculis præcedentibus. Nunc ſane ſæpius acceſſus aquarum Venetiis pertingunt ad ea loca, ad quæ nunquam antea perveniebant. Itaque neceſſe eſt hodie attollere ſtrata viarum, ac præcipue ciſternarum, quæ aquas pluvias excipiunt, ne in magnis æſtubus ab aquâ ſalſâ inficiantur. Nec mirum ſit, in lacunâ urbem ambiente, exiſtere tractus non exiguos qui ab æſtu fere jam nunquam regantur; ſolum enim lacunæ elatum eſt, atque extollitur quotidie (licet magnis ac prorſus regiis ſumptibus omnes fluvii a lacunâ jamdiu depulſi fuerint) ob ſedimenta aquarum, ac maris alluviones.

Ad æſtus quod attinet, ex diario domini TEMANZA, atque ex ſolutis quibuſdam ſchedis, erui menſuram mediam æſtûs, annis hiſce, prout in appoſita tabellâ.

Æſtus medius annorum.

Anni	Æſtus medius. ped.	poll.		
1751	1	11,82		
1752	1	10,53		
1753	2	0,35	1	11,66
1754	1	11,88		
1755	1	11,71		
1760	2	1,11		
1764	2	3,32		
1765	2	0,57	2	1,26
1766, Jun.	2	0,16		
1769	2	1,13		

Si quinque poſteriorum annorum obſervationes fideles eſſent, ut priores (quod tamen ne ipſe quidem D. TEMANZA omnino ſpondet) manifeſto evincerent, æſtum maris Venetiis, annis hiſce proximis, ſummatim creſcere. Quod credibiles reddit obſervationes ipſas eſt id, quod modo aiebam, aquas hoc tempore, quâcunque de cauſâ, inſolitos acceſſus facere, et inundare ea loca quæ numquam antea attingebant. Satis de hâc quæſtione.

15. Modo, datâ occasione, non est tacendus cursus quidam generalis aquarum in toto Adriatico, qui est extra controversiam, et cujus cognitio non inutilis esse potest ratione navigationis. Detectus est ergo cursus quidam, seu motus aquæ, qui ingreditur sinum nostrum a dextrâ parte, sitque secundum littora Epiri, Dalmatiæ, atque Histriæ; torquetur per sinum Tergestinum, refluitque radendo oras Forojulienses, Venetas, Ravennates, reliquas in ditione pontificiâ ac regni Neapolitani, egrediturque a parte sinisttrâ. Hinc nautæ nostri, cum a Corcyrâ atque Ionio, Venetias petunt, legere consueverunt Epiri ac Dalmatiæ littora; cum contra Venetiis Corcyram tendunt, navigare student secundum littora pontificia ac Neapolitana; contenduntque, eodem vento, ac ceteris paribus, hâc viâ multo plus itineris confici, quam secus.

16. Circularem hunc aquarum in Adriatico fluxum detexit primus, ni fallor, inter sciptores GEMINIANUS MONTANARIUS, professor Patavinus, anno 1681, dum jussu publico lacunam visitaret. Rem litteris tradidit, ut videre est in ejus commentario Italico, cui titulus, *Il Mare Adriatico, e sua corrente esaminata*, in *Collectione Autorum qui de Aquis currentibus scripserunt*, vol. iv. recentis editionis Florentinæ, 1768. Ex progressu corporum aquæ innatantium (puta insularum quarumdam ex putrefactis radicibus arundinum palustrium concretarum, quarum

3 plurimæ

plurimæ concrefcunt in paludibus Adrianis) arguit MONTANARIUS, curfum hunc aquæ in Adriatico conficere milliaria tria vel quatuor intra horas xxiiii. At JANUS PLANCUS, qui eundem hunc curfum et agnovit et obfervatione confirmavit, ut videre eft libro laudato, putat, ex progreffu cadaverum fluitantium, quæ aliquando curfu hoc deferuntur ad littora inferiora Ariminenfia, putat, inquam, effe adhuc velociorem.

17. Opinatur porro ingeniofe MONTANARIUS, fluxum hunc in Adriatico derivationem effe curfûs generalis in Mediterraneo. Tefte enim FOURNERIO in *Hydrographiâ*, oceani aquæ, per fretum Gaditanum, Mediterraneum ingrediuntur a parte dexterâ, vel Africæ; hujus littora radunt ufque ad Ægyptum et Syriam; inde convertuntur per oras Afiæ, fortaffe circumeunt Ægeum, legunt oras Peloponnefi, ingrediuntur in Adriaticum eo modo quo diximus, atque inde exeundo, peragrant littora infera Italiæ, Liguriæ, Galliæ, Hifpaniæ, ac tandem per fretum Gaditanum, a parte finiftrâ, in oceanum exonerantur ac revertuntur.

18. Hæc habebam quæ de motu aquarum in finu Veneto proferrem. Non ingratum, opinor, erit accipere conclufa quædam ex obfervationibus ad æftus maritimi theoriam fimul pertinentia, ac doctrinam NEWTONIANAM mirifice illuftrantia.

19. Conferre primum placuit æstus syzigiarum cum æstibus quadraturarum, eductis numeris mediis æstuum quinque dierum circa singulas, elicitisque mediis mediorum singulis illis quinque annis exactis. Et quia Luna, cum sita est in perigæo, ob viciniam majorem, attollere magis debet aquas, quam in apogæo, hi quoque numeri additi sunt: ecce tabellam.

Æstus medius ratione situs Lunæ.

	in Novil.		Pr. Quad		Plenil.		Ult. Quad.		Perigæo.		Apogæo.	
1751	2	4,5	1	5,4	2	3,1	1	6,4	2	1,5	1	10,0
1752	2	3,6	1	4,2	2	5,8	1	3,5	1	8,5	1	7,1
1753	2	7,6	1	5,9	2	5,8	1	4,2	2	4,1	2	0,6
1754	2	4,2	1	1,3	2	3,8	1	4,1	2	5,5	2	1,5
1755	2	0,6	1	5,8	2	4,0	1	4,1	2	0,9	1	10,8
Med. 5 ann.	2	4,1	1	4,1	2	4,5	1	4,5	2	1,2	1	10,1

Patet, 1°, æstus fizigiarum medios valde excedere æstus quadraturarum; ut si numeri utriusque syzigiæ, et quadraturæ, componantur, æstus medius syzigiarum sit pollicum 28,3, quadraturarum 16,3, in ratione fere 7 : 4. 2°, Animadverti potest, æstum plenilunii plerumque esse paulo majorem æstu novilunii, ut quodammodo magis videatur retrahi a luminaribus massa globi terrestris,

 quam

quam aquæ oceani, utroque in ſitu, in novilunio quidem ſecundum elationem aquarum; in plenilunio vero luminaribus divulſis, et in oppoſita trahentibus, contra niſum aquarum. Hinc enim ſequitur, æſtum plenilunii aliquantulo elatiorem eſſe debere. Utcumque ſit, certe 3° æſtus Lunæ perigeæ, prout ratio poſtulat, ſuperat æſtum Lunæ apogeæ, in ratione 25,2 ad 22, vel circiter 1 ad 7; quæ eſt proportio apparentis diametri Lunaris a perigæo in apogæum.

20. Porro juxta theoriam phyſicam, ratione ſitûs Lunæ et locorum terreſtrium, ſi cætera ſint paria, maximi fieri deberent æſtus, ſpectatâ tellure totâ, cum Luna imminet lineæ æquinoctiali; ratione vero loci peculiaris, aquæ magis attolli debent, cum Luna, declinatione, cognomini latitudini geographicæ ejuſdem loci potitur; minime in oppoſitâ. Confeci ergo ſummas æſtuum, pro diebus quibus Luna morata eſt (quinquennio hoc) in ſingulis zodiaci ſignis: ex his eductos numeros medios expoſui in adjectâ tabellâ.

Tabula æstus maris, secundum XII. signa zodiaci, quatenus refertur ad Lunam.

	Aries.	Taur.	Gem.	Canc.	Leo.	Virgo.	Libra.	Scorp.	Sagitt.	Capric.	Aquar.	Pisces.	Æstus Annuus	Locus perigæi Lun.
1751	2 0,67	2 2,24	2 1,41	1 11,81	1 10,25	1 11,86	2 0,50	1 10,16	1 10,93	1 8,90	2 0,41	2 0,67	1 11,82	Taur. Gem.
1752	2 1,07	1 10,72	1 10,85	1 11,82	1 11,62	1 10,80	1 11,75	1 9,98	1 8,77	1 7,17	1 9,99	1 10,36	1 10,57	Gem. Canc.
1753	2 0,00	1 11,54	1 11,00	1 10,46	2 3,18	2 3,39	2 1,46	1 11,23	1 11,30	1 10,22	2 0,91	1 11,86	2 0,35	Canc. Leo.
1754	1 9,51	1 9,41	1 11,48	2 0,73	2 1,33	2 3,92	2 2,78	1 11,18	1 10,14	1 11,32	1 10,42	2 0,35	1 11,18	Virg. Lib.
1755	1 9,09	1 10,59	1 6,77	1 10,62	1 10,20	1 11,10	1 7,20	2 1,37	2 0,50	1 10,14	1 10,03	1 8,87	1 11,71	Lib. Scorp.
Med.	1 11,27	1 11,30	1 10,70	1 11,49	2 0,12	2 1,03	2 0,14	1 11,04	1 10,73	1 9,55	1 11,15	1 11,20	1 11,67	

Optandum sane esset, præsto esse observationes plurium annorum quam quinque, ut integræ revolutionis nodorum Lunæ, vel saltem apsidum; et bis fuisse peractas diebus singulis, pro utroque accessu ac recessu æstûs. Interim tabula hæc ostendit: 1°, Minimum æstum in mari nostro contingere, prout jubet theoria, Lunâ signum Capricorni obeunte. 2°, Summatim signorum australium æstus minores esse, quam borealium; 3°, æstum Cancri, et australium omnium (exceptâ librâ) et borealium ascendentium esse maximum; sed 4°, superari ab æstu trium signorum descendentium Leonis, Virginis, ac Libræ, quod videtur principiis nostris repugnare, sed res facile explicatur. Primum enim actio Lunæ in oceanum generatim maxima

maxima esse debet cum versatur prope æquatorem; deinde in signis descendentibus, prope æquinoctium libræ, collectio quædam cumulusque aquarum fieri debet, ex præcedente impressione, in signis borealibus (ratione climatis nostri) et ob vim inertiæ in aquis ipsis. Quare æstus circa Virginem ac Libram debent esse generatim maximi, prout sunt in tabulâ.

21. 5°, Adnotavi pro singulis annis locum perigæi Lunaris; ex quo apparet, perigæum ipsum, motu suo, altiores æstus secum quasi trahere a signo in signum, a Tauro ac Geminis, 1751; ad Cancrum, 1752; ad Leonem, 1753; ad Virginem ac Libram, 1754; ad Scorpionem, 1755. Præterea anno 1753, quo anno perigæum proximum erat vertici nostro, æstum annuum videmus fuisse maximum: omnia juxta theoriam.

22. Similes vices, hortante CL. LAMBERTO, academico Berolinensi, ex observationibus quadraginta annorum CL. POLENI, R. S. S. quæ sunt apud me, deprehendi in motu barometri. Sed de hoc egi in *Specimine* meo *Meteorologico*, et in opusculo, cui titulus *Novæ Tabulæ Barometri*, agamque fortasse alias.

Interim opto, hasce qualescumque notitias meas de æstu maris Veneti, a Societate Regiâ non inutiles judicari. Vale, vir præstantissime, ac favere perge addictissimo cultori tuo.

Dabam Patavii, 9 calendas Nov. 1776.

X. *A Letter from Mr.* Peter Wargentin, *F. R. S. Secretary to the Royal Academy of Sciences at* Stockholm, *to the Rev.* Nevil Maſkelyne, *B. D. F. R. S. and Aſtronomer Royal; concerning the Difference of Longitude of the Royal Obſervatories at* Paris *and* Greenwich, *reſulting from the Eclipſes of* Jupiter's *firſt Satellites, obſerved during the laſt Ten Years: to which is added, a Comparative Table of the correſponding Obſervations of the Firſt Satellite, made in the principal Obſervatories.*

Vir plurimum reverende atque celeberrime,

Read Feb. 6, 1777.

PETIISTI in utriſque litteris tuis, "velim obſervationes ſatellitum Jovis, "præcipuè primi, a te GRENOVICI, et a Cl. D. MESSIER "PARISIIS, ab anno 1765 habitas, inter ſe conferre, ut "inde eliciatur vera differentia utriuſque meridiani:" huic deſiderio tuo lubens ſatisfacio.

Tuis obſervationibus primi ſatellitis non niſi 17 correſpondentes MESSIERII obtigerunt; quarum 8 fuerunt immerſiones

immerſiones, et 9 emerſiones. Ipſas obſervationes, inter ſe et cum multis aliis, atque cum poſtremâ editione mearum tabularum (eâ ſcilicet, quam alteri editioni aſtronomiæ ſuæ inſeruit Cl. DELALANDIUS) comparatas, videas in ſubjunctâ appendice, in quâ Paris Cl. indicat obſervationes MESSIERII, in palatio Clugny; Paris O. autem, in ipſo obſervatorio regio habitas. Hîc ſufficiat attuliſſe reſultantes a quovis immerſionum et emerſionum correſpondentium pari, meridianorum differentias.

Ex immerſionibus correſp.	′	″
1765, Dec. 24. prodit diff. merid.	8	35
1769, Apr. 12.	9	31
Apr. 28.	9	58
1772, Jun. 9.	9	59
Jul. 11.	9	23
1774, Sept. 26.	9	49
Oct. 3.	9	26
Oct. 21.	9	47
Media ex imm. merid. differ.	9	39
Med. excluſa prima,	9	42

Ex emerſionibus.	′	″
1767, Apr. 9. differ. merid.	9	49
1768, Mai. 11.	9	29
Jun. 3.	9	33
Jun. 19.	10	16
1769, Mai. 16.	9	26
Jun. 8.	9	45
1770, Aug. 5.	9	11
1773, Oct. 25.	9	23
Nov. 1.	9	39
Media ex emerſionibus,	9	37
Med. excluſa quarta,	9	32

Tu, vir celeberrime, fere ſemper uſus es teleſcopio NEWTONIANO 6 pedum: CL. MESSIER plerumque GREGORIANO $2\frac{1}{2}$ pedum, vel acromatico $3\frac{1}{2}$ pedum, vel aliis

 æquales

æqualis fere potentiæ, quorum neutrum non plusquam centies objectorum diametros amplificat. Æquales ferè inventæ per immersiones et emersiones correspondentes meridianorum differentiæ, satis indicare videntur, ferme æqualis quoque præstantiæ fuisse utriusque observatoris instrumenta. Alium et certiorem modum ea comparandi nescio, præcipuè cum tu [c] nullibi indicaveris potentiam tui NEWTONIANI: nam longitudo sola ambiguum est argumentum.

Per medium itaque deductum ex 7 paribus immersionum et 8 emersionum, invenitur differentia inter meridianum GRENOVICENSEM et observatorium CL. MESSIER, 9′ 38″; vel seclusis uno immersionum uno emersionum pari, utpote quæ nimium a reliquis recedentem, indicant meridianorum differentiam, 9′ 37″; adeoque foret inter observatorium GRENOVICENSE et regium Parisinum 9′ 35″, hoc est 19 secundis major quam hucusque putavimus. Obstupui videns tantam incertitudinem circa veram meridianorum differentiam inter duo præcipua orbis observatoria, eaque vicina, in quibus ingens observationum numerus, annis

(a) The diameter of the aperture of this telescope is 9.4 inches, as is mentioned in the preface to my Greenwich Observations, from 1765 to 1774, published this year. N. M.

plusquam

pluſquam 100, habitus eſt. Quid tum de aliis ſentiendum? Ab unâ parte vix perſuadere mihi poſſum, tantum errorem tamdiu potuiſſe latere; vel comparatas obſervationes eclipſium Solis, Lunæ, occultationum fixarum, &c. tam multum potuiſſe fallere: ab alterâ, egregius 15 bonarum obſervationum conſenſus, haud facile fortuitus, fidem quandam mereri videbatur; niſi obſervationes ſatellitum, ad determinandas accuratè longitudinum differentias, prorſus inepats judicaveris.

Ad tollendum hoc dubium, conſului obſervationes primi ſatellitis, eodem tempore, in ipſis obſervatoriis regiis, quarum non niſi duæ ſunt immerſiones, ſed novem emerſiones.

Immerſ.	′	″
1767, Jan. 12. diff. merid.	9	16
1772, Jul. 11.	9	4
Medium ex hiſce duabus,	9	10

Emerſ.	′	″
1766, Apr. 11.	9	55
1767, Mart. 22.	9	36
Apr. 16.	9	32
Apr. 30.	9	5
Mai. 9.	9	47
Jun. 1.	10	28
Jun. 3.	9	4
1769, Jun. 8.	10	13
1773, Oct. 5.	9	37
Medium ex omnibus,	9	42
excluſis 6â et 8â,	9	32

Harum longe minor eſt conſenſus: ſi tamen ſumatur medium, erit id 9′ 26″ vel 9′ 21″.

Ulterius comparavi ſeptem paria immerſionum et 4 emerſionum, annis 1761—1764, in utroque obſervatorio captarum: medium ex prioribus reperi 9′ 26″, ex poſterioribus 9′ 30″. Et ne quid deeſſet, excuſſi quoque obſervationes ante annum 1700 factas; inter quas 12 immerſiones correſpondentes indicant, per medium, differentiam meridianorum 9′ 57″; ſed 7 emerſiones tantummodo 8′ 45″: ex hoc utroque medio reſultat novum 9′ 21″.

Porro tentavi, mediantibus meis obſervationibus, quorum multæ tuis ſunt correſpondentes, multæ Pariſienſibus, quæſitam ſtabilire meridianorum differentiam. Ego ſemper uſus ſum tubo achromatico DOLLONDIANO 10 pedum, cum oculari, quod objecta 90 amplificat, et valde diſtincta reddit.

Immerſ.

Immerſ. correſp.

	h	′	″
1765, Dec. 1.	1	12	34
(b) 1766, Oct. 10.	1	12	14
1768, Apr. 2.	1	13	10
1769, Mart. 29.	1	11	47
1774, Sept. 12.	1	12	32
Oct. 3.	1	12	12
1775, Oct. 1.	1	12	59
Medium,	1	12	14

Sed tua obſervatio, die 10 Oct. 1766, vix rectè ſe habere poteſt. Illâ neglectâ, manet ex reliquis medium, 1 12 32

Emerſ.

	h	′	″
1766, Mart. 5.	1	12	11
1767, Jun. 1.	1	12	7
1768, Jun. 12.	1	12	16
1769, Mai. 16.	1	11	32
Jun. 8.	1	12	19
1771, Aug. 17.	1	12	0
Sept. 25.	1	12	8
1772, Sept. 27.	1	12	14
1773, Oct. 25.	1	12	31
1774, Dec. 29.	1	12	23
1775, Feb. 22.	1	12	16
Dec. 27.	1	12	11
Medium ex his,	1	12	11

Obſervationes emerſionum ſatis pulchrè conſpirant: medium ex immerſionibus et emerſionibus innuit differentiam meridianorum Grenovicenſis et Stockholmenſis $1^h\ 12'\ 21''$. Perſuaſus ſum eam $1^h\ 12'\ 20''$ vix eſſe minorem. Per medium ex 8 obſervationibus correſponden-

(b) There was a miſtake of twelve hours in ſetting down this obſervation at Greenwich, by the clock keeping ſidereal time, which made an error of $1'\ 49''$ in the reduction to apparent time. The correct time of immerſion is $16^h\ 59'\ 31''$, which happens to agree exactly with Mr. WARGENTIN's calculation, and the difference of longitude of Stockholm and Greenwich by this obſervation is $1^h\ 12'\ 14''$ inſtead of $1^h\ 10'\ 25$ ſet down above; and the mean difference from all the ſeven correſponding immerſions is $1^h\ 12'\ 30''$. See the errata printed with my Obſervations. N. M.

tibus,

tibus, quæ habitæ ſunt annis 1761—1764, emergit 1h 12′ 25″.

Quod attinet differentiam meridianorum obſervatorii Pariſienſis et Stockholmenſis, ex 8 immerſionibus et 13 emerſionibus primi ſatellitis ſimul in utroque notatis ante annum 1760, concluſi eam, per medium, eſſe 1h 2′ 51″. Sed 7 immerſiones et 12 emerſiones correſpondentes, factæ poſt annum 1760, eam paulo minorem, ſcilicet 2′ 47″, reddunt. Nihilominus, cum per obſervationes quarundam eclipſium ſolarium evincere conati ſunt celeberrimi viri, PINGRÉ, DU SEJOURS, et LEXELL, eam paucis ſecundis eſſe majorem, non refragabor aſſumere eam 1h 2′ 55″, quâ ſubtractâ ab inventâ differentiâ obſervatororum Grenovicenſis et Stockholmenſis 1h 12′ 21″, reſtat differentia Grenovicenſis et Pariſienſis 9′ 26″.

Omnes itaque hæ diſquiſitiones teſtantur, differentiam quæſitam majorem eſſe 9′ 16″ quantam hucuſque exiſtimavimus; et ni fallor ad 9′ 25″ proximè accedere; de quâ re tamen totum ad te, vir celeberrime, defero judicium.

Interim hinc apparet, arduum ſane eſſe, præciſas meridianorum differentias, ope obſervationum ſatellitum Jovis, determinare. Fixarum occultationes a Lunâ, probè obſervatæ et excuſſæ, certiorem ſine dubio ſuppeditant methodum.

Valde

Valde miror, quare illi, qui bonum telescopium vel tubum habent, eo non semper in observandis satellitibus utantur, sed jam hoc, jam alio, forte minus præstanti. Observationes multum dubias, vel aëris vitio vel aliam ob caussam, ne quidem in diarium refero, nam nulla observatio præstat malæ. Quantum malæ observationes, pro bonis venditatæ, me confuderint et torserint, dicere non possum.

Quartus, die 8 Februarii hujus anni, eclipsin tantum partialem passus est. Magnum tuæ in me amicitiæ signum id interpretabor, si, quovis anno finito, tuas mecum communicare velis observationes satellitum; nam his, etiam senex, delector. Hanc quoque provinciam, examinandi motus satellitum, mihi, quasi tacito consensu, detulisse videntur collegæ astronomi. Ceterum ingruens ætas, nimiæ occupationes quas secretarii munus injungit, defectus instrumentorum (nam murali nondum instructus sum, quidquid dicat Celeb. LALANDIUS) ut taceam modicas ingenii vires, vetant quo minus aliquid tuâ vel aliorum exspectatione dignum præstare possim [(b)]. Frigus hâc hieme in Sueciâ continuum fuit, a 28 Dec. ad 5 Februarii; sed neutiquam, pro ratione climatis, præter

(c) Notwithstanding the author's modesty, the astronomers of Europe know him better, and lament with him that he is not so well provided with capital instruments as he wishes and deserves to be. N. M.

modum, rigidum: plerumque 5 vel 10 gradum thermometri REAUMURIANI. Diebus tantum 26 et 27 Januarii, ad 17 et 18 gradus exaſperatum fuit. Minor quoque nivis copia apud nos fuit, quam pro ſolito. Miror itaque intenſitatem frigoris, eodem tempore, apud exteros.

Dab. Stockholmiæ, die 19 Mart. 1776.

Obſervationes primi ſatellitis Jovis in præcipuis obſervatoriis habitæ, inter ſe et cum tabulis comparatæ.

Ann.	Temp. M.	D.	Obſervationis. h ′ ″		Calculus. h ′ ″	Diff. Calc. ′ ″		Obſervatorium.
1765.	Sept.	21.	16 53 22	Im.	16 53 15	0 7	—	Stockholm.
	Oct.	23.	13 31 6	—	13 31 30	0 24	+	Tyrnav.
	Dec.	1.	10 40 11	—	10 40 48	0 37	+	Greenwich.
			11 33 25	—	11 33 33	0 8	+	Lund.
			11 50 56	—	11 51 0	0 4	+	Tyrnav.
			11 52 45	—	11 52 59	0 14	+	Stockholm.
		8.	12 31 34	—	12 31 25	0 9	—	Greenw.
		15.	14 22 3	—	14 21 40	0 23	—	Greenw.
			15 31 40	—	15 31 52	0 12	+	Tyrnav.
		17.	10 1 15	—	10 1 26	0 11	+	Stockh.
		22.	16 12 19	—	16 11 47	0 32	—	Greenw.
		24.	10 39 27	—	10 39 19	0 8	—	Greenw.
			10 48 2	—	10 48 37	0 35	+	Paris Cl. *d.*
			11 49 6	—	11 49 31	0 25	+	Tyrnav.
1766.	Jan.	2.	8 8 44	—	8 9 24	0 40	+	Stockh. *d.*
		9.	9 40 16	—	9 40 37	0 21	+	Lund.
		16.	11 50 51	—	11 51 17	0 26	+	Stockh.
		23.	13 40 32	—	13 41 28	0 56	+	Upſala *d.*
		25.	8 8 30	—	8 9 30	1 0	+	Upſala *d.*

Obſervationes

Obſervationes comparatæ primi ſatellitis Jovis.

Ann. M. D.	Temp. Obſervationis. h ′ ″		Calculus. h ′ ″	Diff. Calc. ′ ″	Obſervatorum.
1766. Jan. 25.	8 10 39	Im.	8 11 10	0 31 +	Stockholm.
Febr. 15.	16 6 31	Em.	16 6 14	0 17 —	Upſala.
24.	12 31 20	-	12 31 48	0 28 +	Stockholm.
Mart. 5.	7 43 55	-	7 44 25	0 30 +	Greenw.
	8 55 4	-	8 54 56	0 8 —	Upſala.
	8 56 6	-	8 56 36	0 30 +	Stockh.
	9 45 36	-	9 45 39	0 3 +	Peterſb.
10.	15 20 27	-	15 20 58	0 31 +	Paris. Cl.
	15 20 35	-	15 20 56	0 21 +	Paris. O.
12.	9 49 54	-	9 50 3	0 9 +	Paris. O.
	9 49 56	-	9 50 5	0 9 +	Paris. Cl.
	10 33 12	-	10 33 32	0 20 +	Lund.
	10 50 41	-	10 50 59	0 18 +	Tyrnav.
	10 50 53	-	10 51 18	0 25 +	Upſala.
	10 52 13	-	10 52 58	0 45 +	Stockh.
	11 41 56	-	11 42 1	0 5 +	Peterſb.
19.	12 30 15	-	12 30 8	0 7 —	Lund.
	12 47 52	-	12 47 54	0 2 +	Upſal. *d.*
21.	7 16 36	-	7 16 48	0 12 +	Tyrnav.
26.	13 33 36	-	13 34 13	0 37 +	Greenw.
28.	8 12 33	-	8 12 44	0 11 +	Paris. Cl.
	9 8 56	-	9 8 53	0 3 —	Wien.
Apr. 4.	11 10 7	-	11 10 48	0 41 +	Upſala.
	11 10 21	-	11 10 29	0 8 +	Tyrnav.
	11 12 4	-	11 12 28	0 24 +	Stockh.
11.	11 56 30	-	11 56 59	0 29 +	Greenw.
	12 6 25	-	12 6 15	0 10 —	Paris. O.
	13 2 32	-	13 2 26	0 6 —	Wien.
20.	8 31 18	-	8 31 49	0 31 +	Paris. Cl.
	8 31 37	-	8 31 47	0 10 +	Paris. O.
	9 27 56	-	9 27 58	0 2 +	Wien.
	9 32 30	-	9 32 43	0 13 +	Tyrnav.

Obfervationes comparatæ primi fatellitis Jovis.

Anni.	Temp. M. D.	Obfervationis. h ′ ″		Calculus. h ′ ″	Diff. Calc. ′ ″	Obfervatorium.
1766.	Apr. 20.	9 34 24	Em.	9 34 42	0 18 +	Stockholm.
		10 23 9	–	10 23 45	0 36 +	Peterfb.
	27.	11 30 20	–	11 30 42	0 22 +	Stockh.
	May 13.	9 47 45	–	9 48 2	0 17 +	Tyrnav.
		9 49 24	–	9 50 1	0 37 +	Stockholm.
	20.	11 25 12	–	11 25 4	0 8 —	Lund.
	Jun. 5.	9 54 1	–	9 53 44	0 17 —	Wien.
		9 58 39	–	9 58 29	0 10 —	Tyrnav.
	Oct. 10. *(d)*	17 1 20	Im.	16 59 31	1 49 —	Greenw.
		18 11 45	–	18 11 42	0 3 —	Stockh.
	26.	16 27 59	–	16 28 13	0 14 +	Tyrnav.
	Nov. 2.	17 21 9	–	17 21 18	0 9 +	Paris. Cl.
		18 22 20	–	18 22 12	0 8 —	Tyrnav.
		18 24 19	–	18 24 11	0 8 —	Stockholm.
	18.	16 36 1	–	16 35 51	0 10 —	Tyrnav.
	25.	18 9 27	–	18 10 1	0 34 +	Lund. *d.*
		18 29 37	–	18 29 27	0 10 —	Stockh.
	Dec. 4.	13 44 46	–	13 45 9	0 23 +	Paris. O.
		13 44 53	–	13 45 11	0 18 +	Paris. Cl.
	18.	17 25 29	–	17 25 29	0 0	Paris. O.
1767.	Jan. 3.	15 24 1	–	15 23 57	0 4 —	Greenw.
	12.	11 41 41	–	11 42 15	0 34 +	Greenw.
		11 50 57	–	11 51 31	0 34 +	Paris. O.
	26.	15 34 34	–	15 34 40	0 6 +	Paris. Cl.
	Feb. 2.	17 26 54	–	17 27 20	0 26 +	Paris. Obf.
		17 27 13	–	17 27 22	0 9 +	Paris. Cl.
	13.	9 18 13	–	9 18 44	0 31 +	Tyrnav.
	20.	11 14 44	–	11 15 14	0 30 +	Stockholm.
	27.	11 57 7	–	11 58 6	0 59 +	Greenw. *d.*
	Mart. 17.	8 12 16	Em.	8 11 32	0 44 —	Tyrnav.
	22.	14 28 48	–	14 28 40	0 8 —	Greenw.

(d) This obfervation rightly reduced to apparent time is $16^h\ 59'\ 31''$, which happens to agree exactly with Mr. WARGENTIN's calculation. N. M.

Obfervationes

Obſervationes comparatæ primi ſatellitis Jovis.

Ann.	Temp. Obſervationis. M. D.	h ′ ″		Calculus. h ′ ″	Diff. Calc. ′ ″		Obſervatorium.
1767,	Mar. 22.	14 38 24	Em.	14 37 56	0 28	—	Paris. O.
	31.	12 4 36	-	12 5 0	0 24	+	Upſala.
		12 4 54	-	12 4 41	0 13	—	Tyrnav.
		12 6 28	-	12 6 40	0 12	+	Stockh.
	Apr. 7.	14 1 46	-	14 1 35	0 11	—	Upſala.
		14 2 59	-	14 3 15	0 16	+	Stockh.
	9.	7 20 1	-	7 20 13	0 12	+	Greenw.
		7 29 50	-	7 29 31	0 19	—	Paris. Cl.
	14.	14 47 52	-	14 47 36	0 16	—	Greenw.
	16.	9 16 13	-	9 16 43	0 30	+	Greenw.
		9 25 45	-	9 25 59	0 14	+	Paris. O.
	23.	12 23 22	-	12 23 10	0 12	—	Tyrnav.
		12 23 42	-	12 23 29	0 13	—	Upſala.
		12 24 30	-	12 25 9	0 39	+	Stockh.
	30.	13 9 10	-	13 8 47	0 23	—	Greenw.
		13 18 15	-	13 18 3	0 12	—	Paris. O.
	Mai. 9.	9 32 26	-	9 32 58	0 32	+	Greenw.
		9 42 13	-	9 42 14	0 1	+	Paris. O.
		10 43 7	-	10 43 10	0 3	+	Tyrnav.
	16.	12 38 6	-	12 37 54	0 12	—	Tyrnav.
	30.	10 15 32	-	10 15 17	0 15	—	Philadelphia.
	Jun. 1.	9 44 1	-	9 44 20	0 19	+	Greenw.
		9 54 29	-	9 53 36	0 53	—	Paris. O. *d.*
		10 56 8	-	10 56 31	0 23	+	Stockh.
	8.	11 37 42	-	11 37 49	0 7	+	Greenw.
		11 46 46	-	11 47 5	0 19	+	Paris. O.
	Nov. 21.	19 10 24	Im.	19 10 35	0 11	+	Lund.
	Dec. 23.	14 43 47	-	14 43 45	0 2	—	Paris. Cl.
	30.	16 24 27	-	16 24 16	0 11	—	Greenw.
1768.	Jan. 22.	16 24 13	-	16 23 37	0 36	—	Greenw.
	31.	12 53 11	-	12 52 56	0 15	—	Paris. O.
		12 53 22	-	12 52 58	0 24	—	Paris. Cl.

Obſervationes

Obſervationes comparatæ primi ſatellitis Jovis.

Ann.	Temp. Obſervationis.		Calculus.	Diff. Calc.	Obſervatorium.
M. D.	h ′ ″		h ′ ″	′ ″	
1768. Feb. 14.	16 39 50	Im.	16 39 28	0 22 —	Paris. Cl.
	17 42 37	-	17 42 21	0 16 —	Stockh.
16.	12 8 37	-	12 9 14	0 37 +	Upſala.
	12 8 54	-	12 8 55	0 1 +	Tyrnav.
	12 11 9	-	12 10 54	0 15 —	Stockh.
	12 59 24	-	12 59 57	0 33 +	Peterſburg.
Mart. 1.	9 46 49	-	9 47 26	0 37 +	Philadelphia.
	14 57 57	-	14 57 21	0 36 —	Paris. Cl.
	15 58 20	-	15 58 15	0 5 —	Tyrnav.
3.	10 26 59	-	10 27 4	0 5 +	Tyrnav.
8.	17 54 32	-	17 54 10	0 22 —	Upſala.
10.	11 22 15	-	11 21 56	0 19 —	Paris. Cl.
	12 22 40	-	12 22 50	0 10 +	Tyrnav.
	13 13 25	-	13 13 52	0 27 +	Peterſburg.
17.	13 32 17	-	13 32 47	0 30 +	Geneve.
19.	9 39 0	-	9 38 56	0 4 —	Peterſburg.
24.	15 29 3	-	15 29 3	0 0	Geneve.
26.	9 43 3	-	9 43 16	0 13 +	Paris. O.
	9 58 1	-	9 58 6	0 5 +	Geneve.
	10 43 56	-	10 44 12	0 16 +	Tyrnav.
	11 34 52	-	11 35 14	0 22 +	Peterſburg.
Apr. 2.	11 29 33	-	11 30 25	0 52 +	Greenw. *d.*
	12 42 43	-	12 42 36	0 7 —	Stockh.
18.	12 1 37	Em.	12 1 14	0 23 —	Greenw.
25.	8 56 50	-	8 56 33	0 17 —	Philadelphia.
	13 57 19	-	13 57 10	0 9 —	Greenw.
27.	8 35 11	-	8 35 26	0 15 +	Paris. Cl.
Mai. 4.	10 31 0	-	10 30 56	0 4 —	Paris. Cl.
	11 32 18	-	11 31 50	0 28 —	Tyrnav.
	11 33 22	-	11 33 49	0 27 +	Stockh.
11.	12 16 46	-	12 16 46	0 0	Greenw.
	12 26 15	-	12 26 4	0 11 —	Paris. Cl.

Obſervationes comparatæ primi ſatellitis Jovis.

Ann.	Temp. M D.	Obſervationis. h ′ ″		Calculus. h ′ ″	Diff. Calc. ′ ″	Obſervatorium.
1768.	Mai. 20.	8 49 54	–	8 49 29	0 25 —	Paris. Cl.
		9 52 6	–	9 52 22	0 16 +	Stockh.
	27.	10 43 14	–	10 43 44	0 30 +	Paris. Cl.
	Jun. 3.	12 28 6	–	12 28 19	0 13 +	Greenw.
		12 37 39	–	12 37 37	0 2 —	Paris. Cl.
	12.	8 50 16	–	8 50 18	0 2 +	Greenw.
		10 0 23	–	10 0 49	0 26 +	Upſala.
		10 2 32	–	10 2 29	0 3 —	Stockh.
	19.	10 43 27	–	10 43 38	0 11 +	Greenw.
		10 53 43	–	10 52 56	0 47 —	Paris. Cl. *d.*
	Jul. 5.	9 8 5	–	9 8 13	0 8 +	Paris. Cl.
1769.	Jan. 17.	18 37 12	Im.	18 37 6	0 6 —	Stockh.
	Feb. 2.	16 46 30	–	16 46 19	0 11 —	Tyrnav.
	16.	14 21 10	–	14 20 57	0 13 —	Norriton.
		14 21 51	–	14 21 49	0 2 —	Philadelphia.
	23.	16 15 1	–	16 14 59	0 2 —	Norriton.
		16 16 21	–	16 15 51	0 30 —	Philadelphia.
	Mart. 20.	16 9 9	–	16 9 3	0 6 —	Paris. Cl.
	29.	12 25 7	–	12 24 22	0 45 —	Greenw.
		13 34 34	–	13 34 34	0 0	Tyrnav.
		13 36 54	–	13 36 33	0 21 —	Stockh.
	Apr. 3.	14 49 25	–	14 49 49	0 24 +	Norriton.
		14 50 48	–	14 50 41	0 7 —	Philadelphia.
	5.	15 13 35	–	15 13 1	0 34 —	Lund.
		15 32 30	–	15 32 27	0 3 —	Stockh.
	10.	16 46 0	–	16 45 41	0 19 —	Norriton.
	12.	11 14 37	–	11 14 40	0 3 +	Norriton.
		11 15 49	–	11 15 32	0 17 —	Philadelphia.
		16 16 8	–	16 16 9	0 1 +	Greenw.
		16 25 39	–	16 25 27	0 12 —	Paris. Cl.
	21.	12 50 14	–	12 50 1	0 13 —	Paris. Cl.
		13 52 41	–	13 52 54	0 13 +	Stockh. *d.*

Obſervationes

Obſervationes comparatæ primi ſatellitis Jovis.

Ann.	Temp. M. D.	Obſervationis h ′ ″		Calculus. h ′ ″	Diff. Calc. ′ ″	Obſervatorium.
1769.	Apr. 28.	14 35 17	–	14 36 7	0 50 +	Greenw.
		14 45 15	–	14 45 25	0 10 +	Paris. Cl.
	30.	9 13 42	–	9 14 16	0 34 +	Paris. O.
		10 16 38	–	10 17 9	0 31 +	Stockh.
	Mai. 5.	11 29 27	–	11 29 43	0 16 +	Norriton.
		11 30 28	–	11 30 35	0 7 +	Philadelphia.
	12.	10 37 6	Em.	10 35 37	1 29 —	Otaheite. *d.*
	16.	9 31 35	–	9 30 54	0 41 —	Greenw.
		9 41 1	–	9 40 12	0 49 —	Paris. Cl. *d.*
		10 43 7	–	10 43 5	0 2 —	Stockh.
	21.	11 55 13	–	11 55 5	0 8 —	Norriton.
	23.	11 34 52	–	11 34 28	0 24 —	Paris. Cl.
		12 37 42	–	12 37 21	0 21 —	Stockh.
	28.	11 31 59	–	11 31 53	0 6 —	S. Joſeph.
	Jun. 4.	10 45 31	–	10 46 31	1 0 +	Otaheite.
	6.	7 53 58	–	7 54 8	0 10 +	S. Joſeph.
		10 11 32	–	10 11 27	0 5 —	Norriton.
	8.	9 40 56	–	9 41 20	0 24 +	Greenw.
		9 50 41	–	9 50 38	0 3 —	Paris. Cl.
		9 51 9	–	9 50 36	0 33 —	Paris. O.
		10 51 45	–	10 51 51	0 6 +	Upſala.
		10 53 15	–	10 53 31	0 16 +	Stockh.
	13.	7 8 16	–	7 8 28	0 12 +	Otaheite.
		12 5 1	–	12 5 1	0 0	Norriton.
	15.	11 35 33	–	11 34 53	0 40 —	Greenw.
	18.	14 33 36	–	14 33 37	0 1 +	Otaheite.
	20.	9 1 43	–	9 1 57	0 14 +	Otaheite.
		11 40 56	–	11 41 11	0 15 +	Norriton.
	22.	8 27 35	–	8 27 51	0 16 +	Philadelphia.
	24.	9 6 41	–	9 7 3	0 22 +	Tyrnav.
	27.	10 56 15	–	10 55 39	0 36 —	Otaheite.
	29.	8 2 52	–	8 3 14	0 22 +	S. Joſeph.

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Obſervationes comparatæ primi ſatellitis Jovis.

Ann.	M. D.	Temp. Obſervationis. h ′ ″		Calculus. h ′ ″	Diff. Calc. ′ ″	Obſervatorium.
1769.	June 29.	10 21 55	Em.	10 21 25	0 30 —	Philadelph.
	Jul. 1.	9 50 24	–	9 50 31	0 7 +	Greenwich.
		11 0 59	–	11 0 43	0 16 —	Tyrnav.
	6.	7 18 16	–	7 17 56	0 20 —	Otaheite.
	13.	11 51 49	–	11 51 24	0 25 —	S. Joſeph.
	24.	10 12 28	–	10 11 41	0 47 —	Paris. Cl.
	Aug. 23.	7 15 48	–	7 15 14	0 34 —	Philadelph.
1770.	Jan. 29.	17 52 18	Im.	17 52 29	0 11 +	Tyrnav.
	Mart. 16.	17 2 47	–	17 2 16	0 31 —	Greenwich.
	25.	14 37 14	–	14 36 33	0 41 —	Tyrnav.
	Mai. 3.	13 9 36	–	13 9 33	0 3 —	Tyrnav.
	10.	14 45 45	–	14 46 20	0 35 +	Lund.
		15 5 43	–	15 5 46	0 3 +	Stockholm.
	26.	13 2 18	–	13 3 9	0 51 +	Berlin.
	Jun. 4.	9 40 27	–	9 41 18	0 51 +	Tyrnav.
	11.	12 35 22	Em.	12 34 27	0 55 —	Greenwich.
	20.	10 8 30	–	10 8 6	0 24 —	Stockholm.
	Jul. 13.	9 5 7	–	9 5 19	0 12 +	Greenwich.
		9 6 24	–	9 5 38	0 46 —	Chiſlehurſt.
		9 59 15	–	9 59 1	0 14 —	Berlin.
	29.	8 34 19	–	8 34 13	0 6 —	Tyrnav.
	Aug. 5.	9 19 41	–	9 19 57	0 16 +	Greenwich.
		9 20 42	–	9 20 16	0 26 —	Chiſlehurſt.
		9 28 52	–	9 29 12	0 20 +	Paris. Cl.
		10 13 31	–	10 13 34	0 3 +	Berlin.
		10 30 25	–	10 30 6	0 19 —	Tyrnav.
	21.	8 52 49	–	8 52 47	0 2 —	Tyrnav.
	Sept. 13.	8 15 14	–	8 14 46	0 28 +	Paris. Cl.
1771.	Mart. 28.	16 45 22	Im.	16 45 6	0 16 —	Paris. Cl.
	Apr. 13.	15 4 21	–	15 4 16	0 5 —	Paris. Cl.
	Mai. 22.	13 46 13	–	13 46 28	0 15 +	Geneve.

Obſervationes comparatæ primi ſatellitis Jovis.

Ann.	Temp. Obſervationis.					Calculus.			Diff. Calc.			Obſervatorium.
	M. D.	h	′	″		h	′	″	′	″		
1771.	Mai. 22.	14	32	20	Im.	14	32	25	0	5	+	Tyrnav.
	Jun. 7.	11	44	57	–	11	44	53	0	4	—	Paris. Cl.
		11	59	43	–	11	59	50	0	7	+	Geneve.
	14.	13	52	20	–	13	52	14	0	6	—	Geneve.
		14	22	4	–	14	21	39	0	25	—	Berlin.
	Jul. 23.	14	5	38	Em.	14	4	58	0	40	—	Greenwich.
	25.	9	44	15	–	9	43	51	0	24	—	Tyrnav.
	Aug. 1.	10	37	38	–	10	38	1	0	23	+	Paris Cl.
	10.	8	3	49	–	8	3	38	0	11	—	Tyrnav.
	17.	8	49	47	–	8	49	53	0	6	+	Greenw.
		8	51	0	–	8	50	12	0	48	—	Chiſlehurſt.
		9	14	12	–	9	14	8	0	4	—	Geneve.
		10	1	47	–	10	2	4	0	17	+	Stock.
	Sept. 2.	8	23	55	–	8	24	2	0	7	+	Tyrnav.
		8	26	14	–	8	26	1	0	13	—	Stockholm.
	9.	9	11	56	–	9	11	44	0	12	—	Greenw.
		9	12	18	–	9	12	3	0	15	—	Chiſlehurſt.
	25.	7	37	43	–	7	37	24	0	19	—	Greenw.
		8	49	51	–	8	49	35	0	16	—	Stockh.
	Oct. 2.	9	35	40	–	9	35	26	0	14	—	Greenw.
		9	35	56	–	9	35	45	0	11	—	Chiſleh.
	11.	6	2	33	–	6	2	39	0	6	+	Greenw.
		6	3	16	–	6	2	58	0	18	—	Chiſleh.
		7	13	21	–	7	12	47	0	34	—	Tyrnav.
	Nov. 3.	7	32	32	–	7	32	12	0	20	—	Tyrnav.
	19.	5	52	27	–	5	52	34	0	7	+	Stockh.
1772.	Mai. 12.	15	11	30	Im.	15	12	1	0	31	+	Pekin.
	Jun. 9. *	14	57	33	–	14	57	14	0	19	—	Greenw.
		15	7	32	–	15	6	32	1	0	—	Paris. Cl.
		15	28	20	–	15	27	55	0	25	—	Perinaldo.
	25.	13	9	17	–	13	18	43	0	34	—	Paris. O.

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Obſervationes comparatæ primi ſatellitis Jovis.

Ann.	Temp. Obſervationis. M. D.	h ′ ″		Calculus. h ′ ″	Diff. Calc. Obſervationum. ′ ″	
1772.	Jun. 25.	13 40 9	Im.	13 40 8	0 1 —	Perinaldo.
		14 20 1	—	14 19 39	0 22 —	Tyrnav.
	27.	15 22 54	—	15 23 10	0 16 +	Pekin.
	Jul. 2.	15 32 31	—	15 32 27	0 4 —	Perinaldo.
	11.	11 22 34	—	11 22 33	0 1 —	Greenw.
		11 22 34	—	11 22 52	0 18 +	Chiſlehurſt.
		11 31 38	—	11 31 49	0 11 +	Paris. O.
		11 31 57	—	11 31 51	0 6 —	Paris. Cl.
		12 32 25	—	12 32 45	0 20 +	Tyrnav.
	18.	13 25 10	—	13 25 4	0 6 —	Paris. O.
		14 25 41	—	14 25 59	0 18 +	Tyrnav.
	27.	9 38 2	—	9 38 25	0 23 +	Chiſlehurſt.
		9 47 28	—	9 47 24	0 4 —	Paris. Cl.
		11 39 6	—	11 39 20	0 14 +	Peterſb.
	Aug. 3.	11 32 48	—	11 33 2	0 14 +	Chiſlehurſt.
		13 33 40	—	13 33 57	0 17 +	Peterſb.
	10.	13 36 46	—	13 37 10	0 24 +	Paris. O.
		13 37 11	—	13 37 12	0 1 +	Paris. Cl.
		15 28 26	—	15 29 8	0 42 +	Peterſb.
	21.	14 23 21	Em.	14 22 41	0 40 —	Pekin.
	26.	14 4 22	—	14 4 18	0 4 —	Greenw.
	28.	9 26 55	—	9 27 20	0 25 +	Berlin.
		9 44 5	—	9 43 52	0 13 —	Tyrnav.
	30.	10 48 44	—	10 48 31	0 13 —	Pekin.
	Sept. 4.	11 23 38	—	11 23 26	0 12 —	Lund.
		11 23 50	—	11 24 21	0 29 +	Berlin.
		11 41 11	—	11 40 53	0 18 —	Tyrnav.
	8.	7 15 19	—	7 14 59	0 20 —	Pekin.
	13.	8 9 44	—	8 9 38	0 6 —	Stockholm.
	15.	9 12 7	—	9 12 29	0 22 +	Pekin.
	20.	9 47 54	—	9 47 49	0 5 —	Lund.

Obſervationes comparatæ primi ſatellitis Jovis.

Anni.	Temp. M.	D.	Obſervationis. h ′ ″		Calculus. h ′ ″	Diff. Calc. ′ ″		Obſervatorium.
1772.	Sept.	20.	10 6 56	Em.	10 7 16	0 20	+	Stockh.
		22.	11 9 47	–	11 10 6	0 19	+	Pekin.
		27.	10 52 31	–	10 52 47	0 16	+	Greenw.
			12 2 52	–	12 3 18	0 26	+	Upſala.
			12 4 45	–	12 4 58	0 13	+	Stockh.
	Oct.	4.	12 50 41	–	12 50 41	0 0		Chiſlehurſt.
		6.	7 28 53	–	7 29 6	0 13	+	Paris. O.
			8 13 17	–	8 13 30	0 13	+	Berlin.
			8 30 12	–	8 30 2	0 10	—	Tyrnav.
		13.	9 16 49	–	9 17 13	0 24	+	Greenw.
			9 17 13	–	9 17 32	0 19	+	Chiſlehurſt.
			10 9 40	–	10 9 58	0 18	+	Lund.
			10 10 13	–	10 10 53	0 40	+	Berlin.
			10 27 31	–	10 27 25	0 6	—	Tyrnav.
		17.	6 1 36	–	6 1 26	0 10	—	Pekin.
		20.	11 13 59	–	11 14 11	0 12	+	Greenw.
			11 14 32	–	11 14 30	0 2	—	Chiſlehurſt.
		22.	6 36 57	–	6 36 59	0 2	+	Berlin.
			6 53 28	–	6 53 31	0 3	+	Tyrnav.
		24.	7 58 20	–	7 58 10	0 10	—	Pekin.
		29.	8 50 14	–	8 49 55	0 19	+	Tyrnav.
		31.	9 54 21	–	9 54 25	0 4	+	Pekin.
	Nov.	9.	6 19 6	–	6 18 51	0 15	—	Pekin.
		14.	5 59 28	–	5 59 23	0 5	—	Greenw.
		16.	8 13 54	–	8 13 37	0 17	—	Pekin.
	Dec.	2.	6 29 46	–	6 29 41	0 5	—	Pekin.
		9.	8 23 12	–	8 22 37	0 35	—	Pekin.
		23.	5 15 3	–	5 14 39	0 24	—	Lund.
			5 15 51	–	5 15 34	0 17	—	Berlin.
		25.	6 36 12	–	6 35 36	0 36	—	Pekin.
1773.	Mai.	29.	15 18 46	Im.	15 18 4	0 42	—	Perinaldo. *d.*

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Ann.	Temp. Obſervationis. M. D.	h ′ ″		Calculus. h ′ ″	Diff. Calc. ′ ″	Obſervatorium.
1773.	Jun. 14.	14 10 37	Im.	14 10 45	0 8 +	Tyrnav.
	16.	15 14 17	–	15 14 13	0 4 —	Pekin.
	21.	15 16 43	–	15 16 58	0 15 +	Geneve.
	Jul. 7.	13 35 59	–	13 36 23	0 24 +	Perinaldo. *d.*
	14.	15 28 55	–	15 28 45	0 10 —	Perinaldo.
	16.	11 27 8	–	11 27 40	0 32 +	Peterſburg.
	23.	11 19 22	–	11 19 42	0 20 +	Greenw.
		11 50 22	–	11 50 17	0 5 —	Perinaldo.
	30.	13 44 28	–	13 44 6	0 22 —	Perinaldo.
		15 14 40	–	15 14 45	0 5 +	Peterſburg.
	Aug. 6.	15 17 30	–	15 17 13	0 17 —	Paris. Cl.
		15 17 36	–	15 17 11	0 25 —	Paris. O.
		15 32 20	–	15 32 10	0 10 —	Geneve.
	8.	10 0 42	–	10 0 53	0 11 +	Geneve.
		10 7 15	–	10 7 13	0 2 —	Perinaldo.
		10 48 41	–	10 48 49	0 8 +	Stockholm.
		11 37 37	–	11 37 52	0 15 +	Peterſb.
	15.	11 41 5	–	11 40 47	0 18 —	Paris. Cl.
		11 55 58	–	11 55 44	0 14 —	Geneve.
		12 41 23	–	12 41 41	0 18 +	Tyrnav.
		13 32 58	–	13 32 43	0 15 —	Peterſburg.
	22.	13 26 14	–	13 27 3	0 49 +	Greenw. *d.*
		15 28 20	–	15 28 17	0 3 —	Peterſb.
	24.	9 8 24	–	9 8 18	0 6 —	Stockholm.
		15 41 22	–	15 41 46	0 24 +	Pekin.
	26.	10 10 31	–	10 10 40	0 9 +	Pekin.
	29.	15 32 41	–	15 32 25	0 16 —	Paris. Cl.
		15 47 40	–	15 47 22	0 18 —	Geneve.
		15 53 49	–	15 53 42	0 7 —	Perinaldo.
	31.	9 51 57	–	9 52 12	0 15 +	Greenw.
		10 22 16	–	10 22 47	0 31 +	Perinaldo.

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Ann.	Temp. Obſervationis. M. D.	h	′	″		Calculus. h	′	″	Diff. Calc. ′	″		Obſervatorium.
1773.	Aug. 31.	11	2	7	Im.	11	2	24	0	17	+	Tyrnav.
	Sept. 7.	12	18	35	–	12	19	11	0	36	+	Perinaldo.
	9.	14	2	56	–	14	3	27	0	31	+	Pekin.
	14.	14	9	8	–	14	9	36	0	28	+	Geneve.
		15	46	14	–	15	46	35	0	21	+	Peterſburg.
	16.	9	27	0	–	9	27	8	0	8	+	Stockholm.
	25.	12	26	7	–	12	26	27	0	20	+	Pekin.
	27.	9	6	28	Em.	9	6	30	0	2	+	Pekin.
	Oct. 2.	9	58	55	–	9	58	41	0	14	—	Tyrnav.
		10	49	49	–	10	49	43	0	6	—	Peterſburg.
		16	33	55	–	16	34	8	0	13	+	Pekin.
	9.	10	44	40	–	10	45	13	0	33	+	Greenw.
		12	46	30	–	12	46	27	0	3	—	Peterſburg.
	11.	7	15	30	–	7	15	36	0	6	+	Peterſb.
		12	59	19	–	13	0	1	0	42	+	Pekin.
	13.	7	28	57	–	7	29	12	0	15	+	Pekin.
	16.	12	50	34	–	12	51	2	0	28	+	Paris. Cl.
		13	12	37	–	13	12	19	0	18	—	Perinaldo.
	18.	7	19	48	–	7	20	5	0	17	+	Paris. Cl.
		7	41	33	–	7	41	22	0	11	—	Perinaldo.
		8	20	52	–	8	20	59	0	7	+	Tyrnav.
	20.	9	25	6	–	9	25	33	0	27	+	Pekin.
	25.	9	6	12	–	9	6	50	0	38	+	Greenw.
		9	15	35	–	9	16	8	0	33	+	Paris. Cl.
		9	15	49	–	9	16	6	0	17	+	Paris. O.
		9	37	1	–	9	37	31	0	30	+	Perinaldo.
		10	17	19	–	10	17	21	0	2	+	Upſala.
		10	18	43	–	10	19	1	0	18	+	Stockholm.
	27.	11	20	54	–	11	21	27	0	33	+	Pekin.
	29.	5	49	54	–	5	50	20	0	26	+	Pekin.
	Nov. 1.	11	2	10	–	11	2	25	0	15	+	Greenw.

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Ann.	Temp. M.	D.	Obſervationis. h ′ ″		Calculus. h ′ ″	Diff. Calc. ′ ″	Obſervatorium.
1773.	Nov.	1.	11 11 49	Em.	11 11 43	0 6 —	Paris. Cl.
			11 33 24	–	11 33 0	0 24 —	Perinaldo.
		3.	6 23 58	–	6 23 58	0 0	Lund.
		26.	6 12 30	–	6 13 3	0 33 +	Perinaldo.
			6 35 2	–	6 35 13	0 11 +	Lund.
			6 52 57	–	6 52 59	0 2 +	Upſala.
			6 54 45	–	6 54 39	0 6 —	Stockh.
			7 44 4	–	7 43 42	0 22 —	Peterſburg.
	Dec.	3.	8 46 25	–	8 46 5	0 20 —	Upſala.
			8 47 58	–	8 47 45	0 13 —	Stockh.
		10.	9 27 59	–	9 28 15	0 16 +	Greenw.
		19.	6 59 15	–	6 59 11	0 4 —	Tyrnav.
1774.	Jan.	2.	9 42 59	–	9 42 55	0 4 —	Paris. Cl.
		4.	5 12 7	–	5 12 4	0 3 —	Tyrnav.
		11.	6 4 24	–	6 4 10	0 14 —	Paris. Cl.
			7 7 2	–	7 7 3	0 1 +	Stockh.
		18.	7 48 22	–	7 48 20	0 2 —	Greenw.
	Feb.	3.	6 15 33	–	6 15 8	0 25 —	Paris. Cl.
			7 16 17	–	7 16 21	0 4 +	Upſala.
			7 18 10	–	7 18 1	0 9 —	Stockh.
		19.	5 38 58	–	5 38 41	0 17 —	Stockh.
		26.	6 23 29	–	6 23 14	0 15 —	Greenw.
	Aug.	4.	12 32 38	Im.	12 32 16	0 22 —	Tyrnav.
			12 32 51	–	12 32 35	0 16 —	Upſala.
			12 34 42	–	12 34 15	0 27 —	Stockh.
			13 23 17	–	13 23 18	0 1 +	Peterſburg.
		18.	15 21 15	–	15 21 3	0 12 —	Paris. Cl.
		20.	10 51 10	–	10 51 6	0 4 —	Upſala.
			10 53 9	–	10 52 46	0 23 —	Stockh.
		27.	12 46 32	–	12 46 44	0 12 +	Upſala.
	Sept.	3.	14 25 0	–	14 24 48	0 12 —	Lund.

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Ann.	Temp. M. D.	Obſervationis h ′ ″		Calculus. h ′ ″	Diff. Calc. ′ ″	Obſervatorium.
1774.	Sept. 3.	14 42 21	Im.	14 42 15	0 6 —	Tyrnav.
		15 33 29	–	15 33 17	0 12 —	Peterſburg.
	10.	15 28 31	–	15 28 7	0 24 —	Greenw.
		16 38 28	–	16 38 19	0 9 —	Tyrnav.
	12.	9 57 14	–	9 57 13	0 1 —	Greenw.
		11 9 46	–	11 9 24	0 22 —	Stockh.
		11 58 36	–	11 58 27	0 9 —	Peterſb.
	21.	8 23 43	–	8 23 43	0 0	Peterſb.
	26.	13 49 53	–	13 49 40	0 13 —	Greenw.
		13 59 42	–	13 58 58	0 44 —	Paris. Cl.
	Oct. 3.	15 46 19	–	15 45 47	0 32 —	Greenw.
		15 55 45	–	15 55 5	0 40 —	Paris. Cl.
		16 58 31	–	16 57 58	0 33 —	Stockh.
	5.	10 24 45	–	10 24 8	0 37 —	Paris. Cl.
		11 25 9	–	11 25 2	0 7 —	Tyrnav.
		11 26 59	–	11 27 1	0 2 +	Stockh. *d.*
		12 16 5	–	12 16 4	0 1 —	Peterſb.
	10.	17 41 57	–	17 41 43	0 14 —	Greenw.
	12.	12 20 25	–	12 20 0	0 25 —	Paris. Cl.
	14.	7 50 7	–	7 50 6	0 1 —	Upſala.
		7 52 0	–	7 51 46	0 14 —	Stockh.
	19.	14 16 20	–	14 15 32	0 48 —	Paris. Cl.
	21.	8 35 0	–	8 35 3	0 3 +	Greenw.
		8 44 47	–	8 44 21	0 26 —	Paris. Cl.
	26.	16 1 23	–	16 1 26	0 3 +	Greenw.
	30.	6 10 47	–	6 10 59	0 12 +	Stockh.
	Nov. 11.	17 19 3	Em.	17 18 53	0 10 —	Lund.
	13.	12 5 2	–	12 4 39	0 23 —	Tyrnav.
	15.	6 15 39	–	6 15 36	0 3 —	Lund.
		6 34 34	–	6 35 2	0 28 +	Stockh.
	20.	12 56 42	–	12 57 3	0 21 +	Paris. Cl.

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Ann. M. D.	Temp. Obſervationis. h ′ ″		Calculus. h ′ ″	Diff. Calc. Obſervationis. ′ ″	
1774. Nov. 22.	8 27 22	Em.	8 28 10	0 48 +	Stockh.
Dec. 6.	11 9 25	–	11 10 7	0 42 +	Paris. C.
	12 11 9	–	12 11 1	0 8 —	Tyrnav.
24.	4 52 25	–	4 52 47	0 22 +	Stockh.
29.	11 3 48	–	11 4 32	0 44 +	Greenw.
	12 14 15	–	12 15 3	0 48 +	Upſala.
	12 16 11	–	12 16 43	0 32 +	Stockh
31.	6 25 5	–	6 25 17	0 12 +	Lund.
1775. Jan. 23.	6 48 55	–	6 49 43	0 48 +	Tyrnav.
Feb. 15.	5 53 2	–	5 53 32	0 30 +	Greenw.
22.	7 49 37	–	7 49 59	0 22 +	Greenw.
	8 42 39	–	8 42 44	0 5 +	Lund.
	9 0 6	–	9 0 11	0 5 +	Tyrnav.
	9 1 55	–	9 2 10	0 15 +	Stockh.
Mart. 10.	7 23 51	–	7 23 53	0 2 +	Tyrnav.
17.	8 11 7	–	8 11 21	0 14 +	Greenw.
Jul. 15.	14 42 36	Im.	14 42 14	0 22 —	Greenw.
24.	13 5 40	–	13 5 46	0 6 +	Peterſb.
Aug. 7.	14 53 55	–	14 53 26.	0 29 —	Greenw.
16.	13 18 25	–	13 18 29	0 4 +	Peterſb.
Sept. 1.	11 38 30	–	11 38 45	0 15 +	Peterſb.
15.	15 31 7	–	15 30 54	0 13 —	Peterſb.
24.	11 6 50	–	11 6 56	0 6 +	Stockh.
Oct. 1.	11 50 3	–	11 50 36	0 33 +	Greenw.
	13 3 2	–	13 2 47	0 15 —	Stockh.
10.	9 27 23	–	9 27 20	0 3 —	Stockh.
22.	17 37 1	–	17 36 25	0 36 —	Greenw.
Nov. 2.	8 27 48	–	8 27 35	0 13 —	Greenw.
18.	7 54 22	–	7 54 5	0 17 —	Stockh.
Dec. 11.	10 6 5	Em.	10 6 34	0 29 +	Stockh.
20.	6 23 33	–	6 23 37	0 4 +	Upſala.

Obſervationes comparatæ primi ſatellitis Jovis.

Ann.	Temp. Obſervationis M. D.	h	′	″		Calculus. h	′	″	Diff. Calc. ′	″		Obſervatorium.
1775.	Dec. 20.	6	24	55	Em.	6	25	17	0	22	+	Stockh.
	27.	7	3	6	–	7	4	9	1	3	+	Greenw.
		8	15	17	–	8	16	20	1	3	+	Stockh.
1776.	Jan. 12.	6	26	48	–	6	27	22	0	34	+	Stockh.
	19.	8	19	7	–	8	19	51	0	44	+	Stockh.
	26.	10	12	15	–	10	13	7	0	52	+	Stockh.
	28.	4	40	51	–	4	41	32	0	41	+	Stockh.
	Febr. 2.	12	6	20	–	12	7	7	0	47	+	Stockh.
	27.	6	51	5	–	6	51	33	0	28	+	Stockh.

XIV. *A Method of finding the Value of an infinite Series of decreasing Quantities of a certain Form, when it converges too slowly to be summed in the common Way by the mere Computation and Addition or Subtraction of some of its initial Terms.* By Francis Maseres, *Esquire, F. R. S. Cursitor Baron of the Exchequer.*

Read Feb. 13, 1777.

ARTICLE Ist. Let a, b, c, d, e, f, g, h, &c. *ad infinitum*, represent a decreasing progression of numbers, so that b shall be less than a, and c than b, and d than c, and so on of the following numbers, *ad infinitum*.

And 2dly, let these numbers be so related to each other, that they not only shall form a decreasing progression themselves, but that their differences, $a-b$, $b-c$, $c-d$, $d-e$, $e-f$, $f-g$, $g-h$, &c. shall also form a decreasing progression, so that $b-c$ shall be less than $a-b$, and $c-d$ than $b-c$, and $d-e$ than $c-d$, and so on of the following differences; and likewise, that the differences of these differences (which may be called *the second differences* of the original numbers a, b, c, d, e, f, g, h, &c. shall form a decreasing progression; and that the differences of those second differences, or *the third differences* of the original numbers

numbers a, b, c, d, e, f, g, h, &c. ſhall alſo form a decreaſing progreſſion; and in like manner, that the differences of the ſaid third differences, or *the fourth differences*, of the original numbers a, b, c, d, e, f, g, h, &c. and the fifth and ſixth differences, and all higher differences, of the ſame numbers, ſhall alſo form decreaſing progreſſions.

And 3dly, let x be a quantity of any magnitude not greater than unity.

Upon theſe ſuppoſitions the value of the infinite ſeries $a-bx+cxx-dx^3+ex^4-fx^5+gx^6-hx^7+$ &c. (in which the ſecond, fourth, ſixth, and eighth, and every following even term, is marked with the ſign $-$, or is to be ſubtracted from that which immediately precedes it) may be determined in the following manner.

Art. 2. Compute the firſt, ſecond, third, fourth, and other ſubſequent differences of the co-efficients of the powers of x in this ſeries, that is, of the numbers b, c, d, e, f, g, h, &c. as far as ſhall be convenient. Theſe differences will be as follows.

Firſt differences, $b-c$, $c-d$, $d-e$, $e-f$, $f-g$, $g-h$, &c.

Second differences,

$b-c-\overline{c-d}$, $c-d-\overline{d-e}$, $d-e-\overline{e-f}$, $e-f-\overline{f-g}$, $f-g-\overline{g-h}$, &c.

or, $b-2c+d$, $c-2d+e$, $d-2e+f$, $e-2f+g$, $f-2g+h$, &c.

Third differences,

$b-2c+d-\overline{c-2d+e}$, $c-2d+e-\overline{d-2e+f}$, $d-2e+f-\overline{e-2f+g}$, $-2f+g-\overline{f-2g+h}$, &c.

or,

or, $b-3c+3d-e$, $c-3d+3e-f$, $d-3e+3f-g$, $e-3f+3g-h$, &c.

Fourth differences, $b-3c+3d-e-\overline{c-3d+3e-f}$,

$$c-3d+3e-f-\overline{d-3e+3f-g},$$

$$d-3e+3f-g-\overline{e-3f+3g-h}, \text{ \&c.}$$

or, $b-4c+6d-4e+f$, $c-4d+6e-4f+g$, $d-4e+6f-4g+h$, &c.

Fifth differences, $b-4c+6d-4e+f-\overline{c-4d+6e-4f+g}$,

$$c-4d+6e-4f+g-\overline{d-4e+6f-4g+h}, \text{ \&c.}$$

or, $b-5c+10d-10e+5f-g$, $c-5d+10e-10f+5g-h$, &c.

Sixth differences,

$$b-5c+10d-10e+5f-g-\overline{c-5d+10e-10f+5g-h}, \text{ \&c.}$$

or, $b-6c+15d-20e+15f-6g+h$, &c.

Let the firſt difference of the firſt order, to wit, $b-c$, be called D^{I};

and the firſt difference of the ſecond order, to wit, $b-2c+d$, be called D^{II};

and the firſt difference of the third order, to wit, $b-3c+3d-e$, be called D^{III};

and the firſt difference of the fourth order, to wit, $b-4c+6d-4e+f$, be called D^{IV};

and the firſt difference of the fifth order, to wit, $b-5c+10d-10e+5f-g$, be called D^{V};

and the firſt difference of the ſixth order, to wit, $b-6c+15d-20e+15f-6g+h$, be called D^{VI};

and in like manner let the firſt differences of the ſeventh, eighth, ninth, and tenth, and every following order of differences be denoted by D^{VII}, D^{VIII}, D^{IX}, D^{X}, &c. that is, by the capital letter D, with a Roman numeral figure annexed;

annexed to it, expreffing the order of differences to which it belongs.

Thefe things being fuppofed, the aforefaid infinite feries $a-bx+cxx-dx^3+ex^4-fx^5+gx^6-hx^7+$ &c. will be equal to the following differential feries, to wit,

$$a-\frac{bx}{1+x}-\frac{\text{D}^{\text{I}}xx}{\overline{1+x}|^2}-\frac{\text{D}^{\text{II}}x^3}{\overline{1+x}|^3}-\frac{\text{D}^{\text{III}}x^4}{\overline{1+x}|^4}-\frac{\text{D}^{\text{IV}}x^5}{\overline{1+x}|^5}-\frac{\text{D}^{\text{V}}x^6}{\overline{1+x}|^6}-\frac{\text{D}^{\text{VI}}x^7}{\overline{1+x}|^7}-\text{\&c.};$$

in which feries all the terms after the firft term a are marked with the fign $-$, or are to be fubtracted from that term.

Art. 3. If we infert the differences themfelves inftead of D^{I}, D^{II}, D^{III}, D^{IV}, D^{V}, &c. in the foregoing differential feries (which it may perhaps fometimes be convenient to do) that feries will be as follows: $a-\frac{bx}{1+x}-\overline{b-c}\times\frac{xx}{\overline{1+x}|^2}$

$$-\overline{b-2c+d}\times\frac{x^3}{\overline{1+x}|^3}-\overline{b-3c+3d-e}\times\frac{x^4}{\overline{1+x}|^4}$$

$$-\overline{b-4c+6d-4e+f}\times\frac{x^5}{\overline{1+x}|^5}$$

$$-\overline{b-5c+10d-10e+5f-g}\times\frac{x^6}{\overline{1+x}|^6}$$

$$-\overline{b-6c+15d-20e+15f-6g+h}\times\frac{x^7}{\overline{1+x}|^7}-\text{\&c. } \textit{ad infinitum}.$$

Of the convergency of the foregoing differential feries.

Art. 4. The foregoing differential feries will always converge with a confiderable degree of fwiftnefs, fo that

that fix or eight of its terms will give the value of the whole (and confequently that of the original feries $a-bx+cxx-dx^3+ex^4-fx^5+gx^6-hx^7+$ &c. to which it is equal) exact to feveral places of figures, even in the moft difficult cafes: for if x is $=1$ (which is its greateft poffible magnitude) $1+x$ will be $=1+1$ or 2, and confequently $\overline{1+x}|^2$, $\overline{1+x}|^3$, $\overline{1+x}|^4$, $\overline{1+x}|^5$, and the following powers of $1+x$, will be equal to 4, 8, 16, 32, and the following powers of 2; and the powers of the fraction $\frac{x}{1+x}$ will be equal to the powers of $\frac{1}{2}$. Therefore the feries $a-\frac{bx}{1+x}-\frac{D^{I}xx}{\overline{1+x}|^2}-\frac{D^{II}x^3}{\overline{1+x}|^3}-\frac{D^{III}x^4}{\overline{1+x}|^4}-\frac{D^{IV}x^5}{\overline{1+x}|^5}-\frac{D^{V}x^6}{\overline{1+x}|^6}-\frac{D^{VI}x^7}{\overline{1+x}|^7}-$&c. will in this cafe be $=$ to $a-\frac{b}{2}-\frac{D^{I}}{4}-\frac{D^{II}}{8}-\frac{D^{III}}{16}-\frac{D^{IV}}{32}-\frac{D^{V}}{64}-\frac{D^{VI}}{128}-$&c. the terms of which decreafe in a greater proportion than that of 1 to 2, becaufe the numerators a, b, D^{I}, D^{II}, D^{III}, D^{IV}, D^{V}, D^{VI}, &c. form a decreafing progreffion, and the denominators increafe in the proportion of 2 to 1.

Of the inveftigation of the foregoing differential feries.

Art. 5. The foregoing differential feries was inveftigated by firft, fuppofing the original feries $a-bx+cxx-dx^3+ex^4-fx^5+gx^6-hx^7+$ &c. to be equal to another feries whofe terms fhould involve the fame powers of x as the former, but in which every power of x fhould be multiplied

multiplied into the ſame power of the fraction $\frac{1}{1+x}$, in order to accelerate their convergency, and then inquiring what would be the co-efficients of the terms of ſuch a ſeries, if ſuch a ſeries is poſſible, and what would be the ſigns to be prefixed to them, or in what manner they would be connected with the firſt term, whether by addition or ſubtraction. In order to this inquiry, I denoted the unknown co-efficients of the aſſumed ſeries by the capital letters P, Q, R, S, T, V, &c. and wrote down the terms of it near each other, without prefixing to them either of the ſigns + and −, but ſeparated them from each other only by a comma; ſo that the fundamental equation, from which I derived the differential ſeries above-mentioned, was as follows: $a-bx+cxx-dx^3+ex^4-fx^5+gx^6-hx^7+$ &c. is $=$ P, $\frac{Qx}{1+x}$, $\frac{Rxx}{\overline{1+x}|^2}$, $\frac{Sx^3}{\overline{1+x}|^3}$, $\frac{Tx^4}{\overline{1+x}|^4}$, $\frac{Vx^5}{\overline{1+x}|^5}$, &c. By neceſſary deductions from this equation it appeared that P would be equal to a; and that all the following terms, of the aſſumed ſeries, to wit, $\frac{Qx}{1+x}$, $\frac{Rxx}{\overline{1+x}|^2}$, $\frac{Sx^3}{\overline{1+x}|^3}$, $\frac{Tx^4}{\overline{1+x}|^4}$, $\frac{Vx^5}{\overline{1+x}|^5}$, &c. muſt be ſubtracted from the firſt term P, or a; and that Q would be equal to $b-c$, or D^{I}; and $R=b-c-\overline{|c-d}$, or $b-2c+d$, or D^{II}; and $S=b-2c+d-\overline{|c-2d+e}$, or $b-3c+3d-e$, or D^{III}; and

T=

T $=b-4c+6d-4e+f$, or D^{IV}; and V $=b-5c+10d-10e+5f-g$, or D^{V}; and fo on of the following co-efficients, to wit, that every new co-efficient of the affumed feries is equal to the firft difference of the next order of the differences derived from the original co-efficients b, c, d, e, f, g, h, &c. And from hence I concluded that the feries $a-bx+cxx-dx^3+ex^4-fx^5+gx^6-hx^7+$ &c. was equal to the feries,

$$a-\frac{bx}{1+x}-\frac{D^{I}xx}{\overline{1+x}|^2}-\frac{D^{II}x^3}{\overline{1+x}|^3}-\frac{D^{III}x^4}{\overline{1+x}|^4}-\frac{D^{IV}x^5}{\overline{1+x}|^5}-\frac{D^{V}x^6}{\overline{1+x}|^6}-\frac{D^{VI}x^7}{\overline{1+x}|^7}-\text{ \&c.}$$

Art. 6. The thought of fuppofing the original feries $a-bx+cxx-dx^3+ex^4-fx^5+$ &c. to be equal to the feries P, $\frac{Qx}{1+x}$, $\frac{Rxx}{\overline{1+x}|^2}$, $\frac{Sx^3}{\overline{1+x}|^3}$, $\frac{Tx^4}{\overline{1+x}|^4}$, $\frac{Vx^5}{\overline{1+x}|^5}$, &c. containing the powers of x multiplied into the fame powers of the fraction $\frac{1}{1+x}$ in order to accelerate their convergency, occurred to me in confequence of reading the late Mr. THOMAS SIMPSON's Mathematical Differtations, p. 62, 63. concerning the fummation of feriefes, in which he makes a fuppofition of a fimilar kind. Yet there feems to be a confiderable difference between his propofition and that which is the fubject of thefe pages; for he feems to fuppofe his quantities p, q, r, s, t, &c. (which anfwer to a, b, c, d, e, &c. in the notation made ufe of in the above feriefes) to form an increafing progreffion of terms, and accordingly fubtracts p from q, and q from r,

and

and r from s, and s from t, and ſo on; and he ſeems alſo to ſuppoſe the differences $q-p$, $r-q$, $s-r$, $t-s$, &c. to form an increaſing progreſſion, and every ſubſequent order of differences to form likewiſe an increaſing progreſſion, and accordingly ſubtracts $q-p$ from $r-q$, and $r-q$ from $s-r$, and $s-r$ from $t-s$, and ſo on; whereas in the foregoing ſeries $a-bx+cxx-dx^3+ex^4-fx^5+gx^6-hx^7$ + &c. the numbers a, b, c, d, e, f, g, h, &c. are ſuppoſed to form a decreaſing progreſſion of terms, as they are moſt commonly found to do in the ſerieſes that occur in the ſolution of mathematical or philoſophical problems.

Examples of the uſefulneſs of the foregoing differential ſeries in finding the values of infinite ſerieſes whoſe terms decreaſe very ſlowly.

Computations of the lengths of circular arcs by means of infinite ſerieſes derived from their tangents.

Art. 7. It is well known, that if r be put for the radius of a circle, and t for the tangent of any arch in it that is not greater than 45°, the magnitude of the arch whoſe tangent is t will be expreſſed by the infinite ſeries $t-\frac{t^3}{3rr}+\frac{t^5}{5r^4}-\frac{t^7}{7r^6}+\frac{t^9}{9r^8}-\frac{t^{11}}{11r^{10}}+\frac{t^{13}}{13r^{12}}-\frac{t^{15}}{15r^{14}}+$ &c. This ſeries converges with great ſwiftneſs when the tangent is

I much

much lefs than the radius; but when the tangent is nearly equal to the radius, it converges exceeding flowly; and when it is quite equal to the radius, or the arch is equal to 45°, the decreafe of the terms is fo flow as to make the computation of it in the common way, by computing the value of its initial terms, abfolutely impracticable. For Sir ISAAC NEWTON has obferved concerning this feries in that extreme cafe (which then becomes equal to $r - \frac{r}{3} + \frac{r}{5} - \frac{r}{7} + \frac{r}{9} - \frac{r}{11} + \frac{r}{13} - \frac{r}{15} +$ &c.) and another feries that is almoft as flow as this, that to exhibit its value exact to twenty decimal places of figures, there would be occafion for no lefs than five thoufand millions of its terms, to compute which would take up above a thoufand years. See Sir ISAAC NEWTON's fecond letter to Mr. OLDENBURGH, dated October 24, 1676, in the *Commercium Epiftolicum*, p. 159. In thefe cafes therefore it will be convenient to make ufe of fome artifice to difcover the value of the feries $t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} + \frac{t^{13}}{13r^{12}} - \frac{t^{15}}{15r^{14}} +$ &c.; and we fhall find the application of the differential feries abovementioned to be a very proper artifice for this purpofe.

Art. 8. In order to make this application, we muft confider the feries $t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} + \frac{t^{13}}{13r^{12}} - \frac{t^{15}}{15r^{14}} +$ &c. as being the product of the multiplication of t into the

 feries

series $1-\frac{tt}{3rr}+\frac{t^4}{5r^4}-\frac{t^6}{7r^6}+\frac{t^8}{9r^8}-\frac{t^{10}}{11r^{10}}+\frac{t^{12}}{13r^{12}}-\frac{t^{14}}{15r^{14}}+$ &c. and must substitute x instead of $\frac{tt}{rr}$ in the terms of this last series, by which means it will be converted into the series $1-\frac{x}{3}+\frac{xx}{5}-\frac{x^3}{7}+\frac{x^4}{9}-\frac{x^5}{11}+\frac{x^6}{13}-\frac{x^7}{15}+$ &c. This series is of the same form with the original series above-mentioned, $a-bx+cxx-dx^3+ex^4-fx^5+gx^6-hx^7+$ &c. the numeral co-efficients $1, \frac{1}{3}, \frac{1}{5}, \frac{1}{7}, \frac{1}{9}, \frac{1}{11}, \frac{1}{13}, \frac{1}{15}$, &c. of the powers of x in the former series answering to the literal or general co-efficients a, b, c, d, e, f, g, h, &c. of the same powers in the latter series. And these numeral co-efficients evidently form a decreasing progression, as the co-efficients a, b, c, d, e, f, g, h, &c. are supposed to do; and we shall find, upon examination, that the differences of these numeral co-efficients, of the several successive orders, also constitute decreasing progressions, as the several successive orders of differences of the co-efficients a, b, c, d, e, f, g, h, &c. are supposed to do. Consequently the series $1-\frac{x}{3}+\frac{xx}{5}-\frac{x^3}{7}+\frac{x^4}{9}-\frac{x^5}{11}+\frac{x^6}{13}-\frac{x^7}{15}+$ &c. will be equal to the differential series

$$a-\frac{bx}{1+x}-\frac{D^{I}xx}{\overline{1+x}|^2}-\frac{D^{II}x^3}{\overline{1+x}|^3}-\frac{D^{III}x^4}{\overline{1+x}|^4}-\frac{D^{IV}x^5}{\overline{1+x}|^5}-\frac{D^{V}x^6}{\overline{1+x}|^6}-\frac{D^{VI}x^7}{\overline{1+x}|^7}-\text{ &c.}$$

if we suppose the letters a, b, c, d, e, f, g, h, &c. to be equal to the numbers $1, \frac{1}{3}, \frac{1}{5}, \frac{1}{7}, \frac{1}{9}, \frac{1}{11}, \frac{1}{13}, \frac{1}{15}$, &c. and $D^{I}, D^{II}, D^{III}, D^{IV}, D^{V}, D^{VI}$, &c. to be the first differences of the

the ſeveral orders of differences of thoſe numbers, beginning from the ſecond term $\frac{1}{3}$. Now the values of theſe numbers, 1, $\frac{1}{3}$, $\frac{1}{5}$, $\frac{1}{7}$, $\frac{1}{9}$, $\frac{1}{11}$, $\frac{1}{13}$, $\frac{1}{15}$, &c. and of their differences of the ſeveral ſucceſſive orders, beginning from the ſecond term $\frac{1}{3}$, will, when expreſſed in decimal fractions, be as follows:

1 is	=	1.000,000,000,000;
$\frac{1}{3}$	=	.333,333,333,333;
$\frac{1}{5}$	=	.200,000,000,000;
$\frac{1}{7}$	=	.142,857,142,857;
$\frac{1}{9}$	=	.111,111,111,111;
$\frac{1}{11}$	=	.090,909,090,909;
$\frac{1}{13}$	=	.076,923,076,923;
$\frac{1}{15}$	=	.066,666,666,666.

The differences of theſe numbers, beginning from the ſecond term, .333,333,333,33, are as follows:

Firſt differences.	Second differences.
.133,333,333,333;	.076,190,476,190;
.057,142,857,143;	.025,396,825,397;
.031,746,031,746;	.011,544,011,544;
.020,202,020,202;	.006,216,006,216;
.013,986,013,986;	.003,729,603,729;
.010,256,410,257;	&c.
&c.	

Third

Third differences.

.050,793,650,793;
.013,852,813,853;
.005,328,005,328;
.002,486,402,487;
&c.

Fourth differences.

.036,940,836,940;
.008,524,808,525;
.002,841,602,841;

&c.

Fifth differences.

.028,416,028,415;
.005,683,205,684;
&c.

Sixth differences.

.022,732,822,731;

&c.

Therefore D^{I} is = .133,333,333,333;
D^{II} = .076,190,476,190;
D^{III} = .050,793,650,793;
D^{IV} = .036,940,836,940;
D^{V} = .028,416,028,415;
D^{VI} = .022,732,822,731.

Therefore the series $1 - \frac{x}{3} + \frac{xx}{5} - \frac{x^3}{7} + \frac{x^4}{9} - \frac{x^5}{11} + \frac{x^6}{13} - \frac{x^7}{15} +$ &c. is equal to the series

$$1 - .333,333,333,333, \times \frac{x}{1+x}$$

$$- .133,333,333,333, \times \frac{xx}{\overline{1+x}|^2}$$

$$- .076,190,476,190, \times \frac{x^3}{\overline{1+x}|^3}$$

$$- .050,793,650,793, \times \frac{x^4}{\overline{1+x}|^4}$$

– .036,

$$- .036,940,836,940, \times \frac{x^5}{\overline{1+x}|^5}$$

$$- .028,416,028,415, \times \frac{x^6}{\overline{1+x}|^6}$$

$$- .022,732,822,731, \times \frac{x^7}{\overline{1+x}|^7}$$

– &c.; and confequently the product of this latter feries into the tangent t will be equal to the product of the former feries $1 - \frac{x}{3} + \frac{xx}{5} - \frac{x^3}{7} + \frac{x^4}{9} - \frac{x^5}{11} + \frac{x^6}{13} - \frac{x^7}{15} +$ &c. into the fame quantity, that is, to the product of the feries $1 - \frac{tt}{3rr} + \frac{t^4}{5r^4} - \frac{t^6}{7r^6} + \frac{t^8}{9r^8} - \frac{t^{10}}{11r^{10}} + \frac{t^{12}}{13r^{12}} - \frac{t^{14}}{15r^{14}} +$ &c. into the tangent t, or to the original feries
$t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} + \frac{t^{13}}{13r^{12}} - \frac{t^{15}}{15r^{14}} +$ &c. which expreffes the magnitude of the arch of which t is the tangent.

Computation of an arch of 30 degrees.

Art. 9. Now let t be the tangent of 30°, which is $= r \times \frac{1}{\sqrt{3}}$. Then will tt be $= \frac{rr}{3}$; and $\frac{tt}{rr}$, or x, $= \frac{rr}{3rr}$, or $\frac{1}{3}$. Therefore $1+x$ will be $= 1 + \frac{1}{3} = \frac{3}{3} + \frac{1}{3} = \frac{4}{3}$, and $\frac{x}{1+x}$ will be $= \frac{\frac{1}{3}}{\frac{4}{3}} = \frac{1}{3} \times \frac{3}{4} = \frac{1}{4}$. Therefore $\frac{xx}{\overline{1+x}|^2}$ will be $= \frac{1}{16}$, and $\frac{x^3}{\overline{1+x}|^3} = \frac{1}{64}$, and $\frac{x^4}{\overline{1+x}|^4} = \frac{1}{256}$, and $x^5 = \frac{1}{1024}$, and $x^6 = \frac{1}{4096}$, and $x^7 = \frac{1}{16384}$. Confequently the differential feries will in this

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case be equal to

$$
\begin{aligned}
1 &- .333,333,333,333, \times \tfrac{1}{4} \\
&- .133,333,333,333, \times \tfrac{1}{16} \\
&- .076,190,476,190, \times \tfrac{1}{64} \\
&- .050,793,650,793, \times \tfrac{1}{256} \\
&- .036,940,836,940, \times \tfrac{1}{1024} \\
&- .028,416,028,415, \times \tfrac{1}{4096} \\
&- .022,732,822,731, \times \tfrac{1}{16384} \\
&- \&c. = 1 - .083,333,333,333, \\
&\qquad - .008,333,333,333, \\
&\qquad - .001,190,476,190, \\
&\qquad - .000,198,412,698, \\
&\qquad - .000,036,075,036, \\
&\qquad - .000,006,937,506, \\
&\qquad - .000,001,387,501, \\
&\qquad - \&c.
\end{aligned}
$$

$$= 1 - .093,099,955,597, = 0.906,900,044,403.$$

Therefore the series $1 - \frac{x}{3} + \frac{xx}{5} - \frac{x^3}{7} + \frac{x^4}{9} - \frac{x^5}{11} + \frac{x^6}{13} - \frac{x^7}{15} + \&c.$ or $1 - \frac{tt}{3rr} + \frac{t^4}{5r^4} - \frac{t^6}{7r^6} + \frac{t^8}{9r^8} - \frac{t^{10}}{11r^{10}} + \frac{t^{12}}{13r^{12}} - \frac{t^{14}}{15r^{14}} + \&c.$ is in this case $= 0.906,900,044,403$, or (neglecting the latter figures after the sixth place of figures, because we are sure they are

are not exact) 0.906,900. Therefore the product of the series $1 - \frac{tt}{3rr} + \frac{t^4}{5r^4} - \frac{t^6}{7r^6} + \frac{t^8}{9r^8} - \frac{t^{10}}{11r^{10}} + \frac{t^{12}}{13r^{12}} - \frac{t^{14}}{15r^{14}} +$ &c. into the tangent t is equal to 0.906,900, $\times t = 0.906,900 \times r \times \frac{1}{\sqrt{3}}$

$$= 0.906,900, \times r \times \frac{1}{1.732,050,8} = 0.906,900 \times r$$

$\times .577,350,2 = 0.523,598,8 \times r$; that is, the series $t - \frac{t^3}{3rr} + \frac{t^5}{5r^5} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} + \frac{t^{13}}{13r^{12}} - \frac{t^{15}}{15r^{14}} +$ &c. (which expresses the magnitude of the arch of which t is the tangent) is in this case $= 0.523,598,8 \times r$, or an arch of 30° is equal to $0.523,598,8 \times r$.

Art. 10. This value of an arch of 30° is exact in the six first places of figures, and errs only an unit in the seventh figure, which should be a 7 instead of an 8, the more exact value of that arch being 0.523,598,775,598, &c. And thus by the help of only eight terms of the differential series

$a - \frac{bx}{1+x} - \frac{D^{I}xx}{\overline{1+x}|^2} - \frac{D^{II}x^3}{\overline{1+x}|^3} - \frac{D^{III}x^4}{\overline{1+x}|^4} - \frac{D^{IV}x^5}{\overline{1+x}|^5} - \frac{D^{V}x^6}{\overline{1+x}|^6} - \frac{D^{VI}x^7}{\overline{1+x}|^7} -$ &c. we have obtained the value of the series

$t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} + \frac{t^{13}}{13r^{12}} - \frac{t^{15}}{15r^{14}} +$ &c. in the case of an arch of 30 degrees, exact to six places of figures. This degree of exactness is the same with that which we should attain by computing twelve terms of the series $t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} + \frac{t^{13}}{13r^{12}} - \frac{t^{15}}{15r^{14}} +$ &c. itself, as will appear from the following calculation.

Art. 11. The series $t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} + \frac{t^{13}}{13r^{12}} - \frac{t^{15}}{15r^{14}} + \frac{t^{17}}{17r^{16}} - \frac{t^{19}}{19r^{18}} + \frac{t^{21}}{21r^{20}} - \frac{t^{23}}{23r^{22}}$ + &c. is $= t \times$ the series $1 - \frac{tt}{3rr} + \frac{t^4}{5r^4} - \frac{t^6}{7r^6} + \frac{t^8}{9r^8} - \frac{t^{10}}{11r^{10}} + \frac{t^{12}}{13r^{12}} - \frac{t^{14}}{15r^{14}} + \frac{t^{16}}{17r^{16}} - \frac{t^{18}}{19r^{18}} + \frac{t^{20}}{21r^{20}} - \frac{t^{22}}{23r^{22}}$ + &c. =, in the case of an arch of 30°, to $r \times \frac{1}{\sqrt{3}}$ into the series $1 - \frac{1}{3 \times 3} + \frac{1}{5 \times 9} - \frac{1}{7 \times 27} + \frac{1}{9 \times 81} - \frac{1}{11 \times 243} + \frac{1}{13 \times 729} - \frac{1}{15 \times 2187} + \frac{1}{17 \times 6561} - \frac{1}{19 \times 19683} + \frac{1}{21 \times 59049} - \frac{1}{23 \times 177,147}$ + &c.

$= r \times \frac{1}{\sqrt{3}} \times$ the series $1.000,000,000,000, - \frac{.333,333,333,333,}{3}$

$+ \frac{.111,111,111,111,}{5} - \frac{.037,037,037,037,}{7}$

$+ \frac{.012,345,679,012,}{9} - \frac{.004,115,226,337,}{11}$

$+ \frac{.001,371,742,112,}{13} - \frac{.000,457,247,370,}{15}$

$+ \frac{.000,152,415,790,}{17} - \frac{.000,050,805,263,}{19}$

$+ \frac{.000,016,935,087,}{21} - \frac{.000,005,645,029,}{23}$

+ &c.

$= r \times \frac{1}{\sqrt{3}} \times$ the series

1.000,000,000,000, − .111,111,111,111,
+ .022,222,222,222, − .005,291,005,291,
+ .001,371,742,112, − .000,374,111,485,
+ .000,105,518,624, − .000,030,483,158,
+ .000,008,965,634, − .000,002,673,961,
+ .000,000,806,432, − .000,000,245,436,
+ &c.

1

$= r \times \frac{1}{\sqrt{3}} \times |1.023,709,255,024, - .116,809,620.442,$
$+$ &c.

$= r \times \frac{1}{\sqrt{3}} \times .906,899,624,582,$

$= r \times \frac{1}{1\ 732,050,8} \times .906,899,624,582,$

$= r \times .577,350,2 \times .906,899,624,582,$ = (if we neglect the six latter figures of 906,899,624,582, which we know to be not exact) $r \times .577,350,2 \times 906,899,$ $= r \times .523,598,319,029,8$; of which the first six figures .523,598, are exact.

Computation of an arch of 45 *degrees.*

Art. 12. Now let the tangent t be equal to the radius r, or the arch (whose magnitude is expressed by the series $t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} + \frac{t^{13}}{13r^{12}} - \frac{t^{15}}{15r^{14}} +$ &c.) be an arch of 45°. This series will, in this case, become equal to $r - \frac{r}{3} + \frac{r}{5} - \frac{r}{7} + \frac{r}{9} - \frac{r}{11} + \frac{r}{13} - \frac{r}{15} +$ &c. of which the first eight terms will give the value of the whole exact to only one figure, as will appear by the following computation. These terms are equal to $r \times$ the eight terms $1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \frac{1}{11} + \frac{1}{13} - \frac{1}{15}$, that is, to $r \times$ the eight terms

$1.000,000,000,000, - .333,333,333,333,$
$+ .200,000,000,000, - .142,857,142,857,$
$+ .111,111,111,111, - .090,909,090,909,$
$+ .076,923,076,923, - .066,666,666,666,$

 =

$=r \times \sqrt{1.388,034,187,034, - .633,766,233,765,}$ $=r \times .754,267,943,269$; which agrees with the value of the whole ſeries $r - \frac{r}{3} + \frac{r}{5} - \frac{r}{7} + \frac{r}{9} - \frac{r}{11} +$ &c. only in the higheſt figure 7, the more exact value of that ſeries being .785,398,163,397, &c. But, if we compute eight terms of the differential ſeries which is equal to the ſeries $r - \frac{r}{3} + \frac{r}{5} - \frac{r}{7} + \frac{r}{9} - \frac{r}{11} +$ &c., we ſhall thereby obtain its value exact to three places of figures; which is as great a degree of exactneſs as would be attained by computing about five hundred terms of the ſeries $r - \frac{r}{3} + \frac{r}{5} - \frac{r}{7} + \frac{r}{9} - \frac{r}{11} + \frac{r}{13} - \frac{r}{15} +$ &c. itſelf. The computation of the eight firſt terms of the ſaid differential ſeries is as follows.

Art. 13. Since t is in this caſe $=r$, tt will be $=rr$, and conſequently $\frac{tt}{rr}$, or x, will be $=1$. Therefore xx, x^3, x^4, x^5, and all the other powers of x, will in this caſe be equal to 1, and $1+x$ will be equal to $1+1$, or 2, and the powers of $1+x$ to the powers of 2. Therefore the fraction $\frac{x}{1+x}$ and its powers will be equal in this caſe to the fraction $\frac{1}{2}$ and its powers. Therefore the general differential ſeries in art. 8. to wit,

$$1 - .333,333,333,333, \times \frac{x}{1+x}$$

$$- .133,333,333,333, \times \frac{xx}{\overline{1+x}|^2}$$

$$- .076,190,476,190, \times \frac{x^3}{\overline{1+x}|^3}$$

$$- .050,793,650,793, \times \frac{x^4}{\overline{1+x}|^4}$$

$$- .036,940,836,940, \times \frac{x^5}{\overline{1+x}|^5}$$

$$- .028,416,028,415, \times \frac{x^6}{\overline{1+x}|^6}$$

$$- .022,732,822,731, \times \frac{x^7}{\overline{1+x}|^7}$$

– &c. will become in this cafe equal to

$$1 - .333,333,333,333, \times \frac{1}{2}$$

$$- .133,333,333,333, \times \frac{1}{4}$$

$$- .076,190,476,190, \times \frac{1}{8}$$

$$- .050,793,650,793, \times \frac{1}{16}$$

$$- .036,940,836,940, \times \frac{1}{32}$$

$$- .028,416,028,415, \times \frac{1}{64}$$

$$- .022,732,822,731, \times \frac{1}{128}$$

$$- \&c. = 1 - .166,666,666,666,$$

$$- .033,333,333,333,$$

$$- .009,523,809,523,$$

$$
\begin{aligned}
&- .003,174,603,174,\\
&- .001,154,401,154,\\
&- .000,444,000,443,\\
&- .000,177,600,177,\\
&- \&c.
\end{aligned}
$$

$$= 1 - .214,474,414,470, = .785,525,585,530.$$

Therefore the ſeries $1 - \frac{x}{3} + \frac{xx}{5} - \frac{x^3}{7} + \frac{x^4}{9} - \frac{x^5}{11} + \frac{x^6}{13} - \frac{x^7}{15} + \&c.$ or $1 - \frac{tt}{3rr} + \frac{t^4}{5r^4} - \frac{t^6}{7r^6} + \frac{t^8}{9r^8} - \frac{t^{10}}{11r^{10}} + \frac{t^{12}}{13r^{12}} - \frac{t^{14}}{15r^{14}} + \&c.$ is equal to $.785,525,585,530$; and conſequently the ſeries $t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} + \frac{t^{13}}{13r^{12}} - \frac{t^{15}}{15r^{14}} + \&c.$ is in this caſe $= t \times .785,525,585,530, = r \times .785,525,585,530$; that is, the length of an arch of 45°, in a circle whoſe radius is r, is $= r \times .785,525,585,530$; which number is true to three places of figures, the more exact value of that arch being $r \times .785,398,163,397$, &c.

Art. 14. It has been aſſerted in art. 12. that in order to obtain the value of the ſeries $t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} + \frac{t^{13}}{13r^{12}} - \frac{t^{15}}{15r^{14}} + \&c.$ exact to 3 places of decimal figures by the mere computation of its terms, in the caſe of an arch of 45°, we muſt compute at leaſt 500 of its terms. This may be proved in the following manner. The indexes of the powers of t in that ſeries are the odd numbers 1, 3, 5, 7, 9, 11, 13, 15, &c.

&c. in their natural order; to which if we add an unit, the numbers thereby produced will be the even numbers 2, 4, 6, 8, 10, 12, 14, 16, &c. in their natural order, which are the doubles of the natural numbers, 1, 2, 3, 4, 5, 6, 7, &c. Therefore the number of terms of that feries from the beginning of it to any given term in it, including the faid term, is always half the number that is produced by adding an unit to the index of t in the faid term. Thus, if we take the term $\frac{t^{11}}{11r^{10}}$, and add 1 to 11, which is the index of the power of t in it, the fum will be 12, the half of which is 6, which is the number of terms in the feries from the beginning of it to the term $\frac{t^{11}}{11r^{10}}$, including the faid term, that term being the fixth term in the feries. If therefore we take the term $\frac{t^{999}}{999r^{998}}$, and are defirous of knowing its place in the feries, or the number of terms from the beginning of the feries to that term inclufively, we muft add 1 to the index of the power of t in its numerator, which will increafe it to 1000; and half this fum, to wit, 500, will be the number of terms from the beginning of the feries to the term $\frac{t^{999}}{999r^{998}}$ inclufively; or, in other words, this term will be the 500th term of the feries. To arrive therefore at thofe terms of the feries in which the in-

dexes

dexes of the powers of t are greater than 999, or 1000, or in which the numeral co-efficients of the terms (which, by the law of this ſeries, are equal to 1 divided by theſe indexes) are leſs than $\frac{1}{999}$ or $\frac{1}{1000}$, it is neceſſary to compute 500 of its terms. Now when t is $= r$, and conſequently the literal parts of the terms of this ſeries do not converge at all, it is evidently neceſſary to carry the computation as far as thoſe terms in which the numeral co-efficients of the terms are leſs than $\frac{1}{999}$ or $\frac{1}{1000}$, in order to get the value of the ſeries exact to the $\frac{1}{999}$th or $\frac{1}{1000}$th part of the radius r, or to the place of thouſandths, or the third place of decimal figures. Therefore, when t is $= r$, or the arch is $= 45°$, it is neceſſary to compute at leaſt 500 terms of the ſeries $t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} + \frac{t^{13}}{13r^{12}} - \frac{t^{15}}{15r^{14}} +$ &c., in order to obtain the value of it exact to three places of decimal figures, that is, to the ſame degree of exactneſs to which we attained in art. 13. by computing only eight terms of the above-mentioned differential ſeries. *Q.E.D.*

Art. 15. But the beſt way of applying the aforeſaid differential ſeries to the inveſtigation of the value of one of theſe very ſlow ſerieſes, is to compute a moderate number of the firſt terms of the ſlow ſeries in the common way, and then apply the differential ſeries to the

computation of its remaining terms. The advantage of this method of proceeding will be manifeſt, if we apply it to the foregoing example of the ſeries $t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} + \frac{t^{13}}{13r^{12}} - \frac{t^{15}}{15r^{14}} +$ &c. in the caſe of an arch of 45°.

Compute therefore the firſt twelve terms of this ſeries in the common way. Theſe terms will be as follows:

$$t = r = r \times 1.000,000,000,000;$$

$$\frac{t^3}{3rr} = \frac{r^3}{3rr} = \frac{r}{3} = r \times .333,333,333,333;$$

$$\frac{t^5}{5r^4} = \frac{r^5}{5r^4} = \frac{r}{5} = r \times .200,000,000,000;$$

$$\frac{t^7}{7r^6} = \frac{r^7}{7r^6} = \frac{r}{7} = r \times .142,857,142,857;$$

$$\frac{t^9}{9r^8} = \frac{r^9}{9r^8} = \frac{r}{9} = r \times .111,111,111,111;$$

$$\frac{t^{11}}{11r^{10}} = \frac{r^{11}}{11r^{10}} = \frac{r}{11} = r \times .090,909,090,909;$$

$$\frac{t^{13}}{13r^{12}} = \frac{r^{13}}{13r^{12}} = \frac{r}{13} = r \times .076,923,076,923;$$

$$\frac{t^{15}}{15r^{14}} = \frac{r^{15}}{15r^{14}} = \frac{r}{15} = r \times .066,666,666,666;$$

$$\frac{t^{17}}{17r^{16}} = \frac{r^{17}}{17r^{16}} = \frac{r}{17} = r \times .058,823,529,411;$$

$$\frac{t^{19}}{19r^{18}} = \frac{r^{19}}{19r^{18}} = \frac{r}{19} = r \times .052,631,578,947;$$

$$\frac{t^{21}}{21r^{20}} = \frac{r^{21}}{21r^{20}} = \frac{r}{21} = r \times .047,619,047,619;$$

$$\frac{t^{23}}{23r^{22}} = \frac{r^{23}}{23r^{22}} = \frac{r}{23} = r \times .043,478,260,869.$$

Therefore the twelve terms $t-\frac{t^3}{3rr}+\frac{t^5}{5r^4}-\frac{t^7}{7r^6}+\frac{t^9}{9r^8}-\frac{t^{11}}{11r^{10}}+\frac{t^{13}}{13r^{12}}-\frac{t^{15}}{15r^{14}}+\frac{t^{17}}{17r^{16}}-\frac{t^{19}}{19r^{18}}+\frac{t^{21}}{21r^{20}}-\frac{t^{23}}{23r^{22}}$ are

$$=\left\{\begin{array}{ll} r\times 1.000,000,000,000, & -r\times .333,333,333,333, \\ +r\times .200,000,000,000, & -r\times .142,857,142,857, \\ +r\times .111,111,111,111, & -r\times .090,909,090,909, \\ +r\times .076,923,076,923, & -r\times .066,666,666,666, \\ +r\times .058,823,529,411, & -r\times .052,631,578,947, \\ +r\times .047,619,047,619, & -r\times .043,478,260,869, \end{array}\right\}$$

$=r\times 1.494,476,765,064,-r\times .729,876,073,581,$

$=r\times \ .764,600,691,483.$

Having thus found the value of the firſt twelve terms of the ſeries $t-\frac{t^3}{3rr}+\frac{t^5}{5r^4}-\frac{t^7}{7r^6}+\frac{t^9}{9r^8}-\frac{t^{11}}{11r^{10}}+$ &c. to be $r\times .764,600,691,483,$ we muſt apply the differential ſeries to the diſcovery of the value of the remaining part of this ſeries, which is the ſeries $\frac{t^{25}}{25r^{24}}-\frac{t^{27}}{27r^{26}}+\frac{t^{29}}{29r^{28}}-\frac{t^{31}}{31r^{30}}+\frac{t^{33}}{33r^{32}}-\frac{t^{35}}{35r^{34}}+\frac{t^{37}}{37r^{36}}-\frac{t^{39}}{39r^{38}}+$ &c. *ad infinitum*. Now this ſeries is equal to the product of $\frac{t^{25}}{r^{24}}$ into the ſeries $\frac{1}{25}-\frac{tt}{27rr}+\frac{t^4}{29r^4}-\frac{t^6}{31r^6}+\frac{t^8}{33r^8}-\frac{t^{10}}{35r^{10}}+\frac{t^{12}}{37r^{12}}-\frac{t^{14}}{39r^{14}}+$ &c. or (putting x, as before, $=\frac{tt}{rr}$) to the product of $\frac{t^{25}}{r^{24}}$ into the

ſeries

series $\frac{1}{25}-\frac{x}{27}+\frac{xx}{29}-\frac{x^3}{31}+\frac{x^4}{33}-\frac{x^5}{35}+\frac{x^6}{37}-\frac{x^7}{39}+$ &c. which is of the same form with the series $a-bx+cxx-dx^3+ex^4-fx^5+gx^6-hx^7+$ &c. Therefore, if we put $a=\frac{1}{25}$, $b=\frac{1}{27}$, $c=\frac{1}{29}$, $d=\frac{1}{31}$, $e=\frac{1}{33}$, $f=\frac{1}{35}$, $g=\frac{1}{37}$, $h=\frac{1}{39}$, and so on, and compute the differential series

$$a-\frac{bx}{1+x}-\frac{D^{I}xx}{\overline{1+x}|^2}-\frac{D^{II}x^3}{\overline{1+x}|^3}-\frac{D^{III}x^4}{\overline{1+x}|^4}-\frac{D^{IV}x^5}{\overline{1+x}|^5}-\frac{D^{V}x^6}{\overline{1+x}|^6}-\frac{D^{VI}x^7}{\overline{1+x}|^7}-\text{ \&c.}$$

thence resulting, the number thereby obtained will be the value of the series $\frac{1}{25}-\frac{x}{27}+\frac{xx}{29}-\frac{x^3}{31}+\frac{x^4}{33}-\frac{x^5}{35}+\frac{x^6}{37}-\frac{x^7}{39}+$ &c. or $\frac{1}{25}-\frac{tt}{27rr}+\frac{t^4}{29r^4}-\frac{t^6}{31r^6}+\frac{t^8}{33r^8}-\frac{t^{10}}{35r^{10}}+\frac{t^{12}}{37r^{12}}-\frac{t^{14}}{39r^{14}}+$ &c. This computation is as follows:

Here a is $=\frac{1}{25}=.040,000,000,000$;

$b \;=\frac{1}{27}=.037,037,037,037$;

$c \;=\frac{1}{29}=.034,482,758,620$;

$d \;=\frac{1}{31}=.032,258,064,516$;

$e \;=\frac{1}{33}=.030,303,030,303$;

$f \;=\frac{1}{35}=.028,571,428,571$;

$g \;=\frac{1}{37}=.027,027,027,027$;

$h \;=\frac{1}{39}=.025,641,025,641$.

 The

The differences of thefe numbers (beginning from the fecond number $\frac{1}{27}$, or .037,037,037,037,) are as follows:

First differences.

.002,554,278,416;
.002,224,694,104;
.001,955,034,213;
.001,731,601,731;
.001,544,401,544;
.001,386,001,386.

Second differences.

.000,329,584,311;
.000,269,659,891;
.000,223,432,481;
.000,187,200,187;
.000,158,400,158.

Third differences.

.000,059,924,420;
.000,046,227,409;
.000,036,232,294;
.000,028,800,028.

Fourth differences.

.000,013,697,010;
.000,009,995,115;
.000,007,432,265.

Fifth differences.

.000,003,701,894;
.000,002,562,850.

Sixth differences.

.000,001,139,044.

Therefore D^{I} is = .002,554,278,416;
D^{II} = .000,329,584,311;
D^{III} = .000,059,924,420;
D^{IV} = .000,013,697,010;
D^{V} = .000,003,701,894;
D^{VI} = .000,001,139,044.

Confequently the differential feries

$$a - \frac{bx}{1+x} - \frac{\text{D}^{\text{I}}xx}{\overline{1+x}|^2} - \frac{\text{D}^{\text{II}}x^3}{\overline{1+x}|^3} - \frac{\text{D}^{\text{III}}x^4}{\overline{1+x}|^4} - \frac{\text{D}^{\text{IV}}x^5}{\overline{1+x}|^5} - \frac{\text{D}^{\text{V}}x^6}{\overline{1+x}|^6} - \frac{\text{D}^{\text{VI}}x^7}{\overline{1+x}|^7} - \text{\&c. is=to}$$

.040,

$$
\begin{aligned}
&.040,000,000,000, \\
&- .037,037,037,037, \times \frac{x}{1+x} \\
&- .002,554,278,416, \times \frac{xx}{\overline{1+x}|^2} \\
&- .000,329,584,311, \times \frac{x^3}{\overline{1+x}|^3} \\
&- .000,059,924,420, \times \frac{x^4}{\overline{1+x}|^4} \\
&- .000,013,697,010, \times \frac{x^5}{\overline{1+x}|^5} \\
&- .000,003,701,894, \times \frac{x^6}{\overline{1+x}|^6} \\
&- .000,001,139,044, \times \frac{x^7}{\overline{1+x}|^7}. \\
&- \&c.
\end{aligned}
$$

But, since t is in this case $= r$, $\frac{tt}{rr}$, or x, will be $= 1$, and consequently $\frac{x}{1+x}$ will be $= \frac{1}{1+1}$ or $\frac{1}{2}$. Therefore the foregoing differential series is in this case equal to

$$
\begin{aligned}
&.040,000,000,000, \\
&- .037,037,037,037, \times \frac{1}{2} \\
&- .002,554,278,416, \times \frac{1}{4} \\
&- .000,329,584,311, \times \frac{1}{8} \\
&- .000,059,924,420, \times \frac{1}{16} \\
&- .000,013,697,010, \times \frac{1}{32}
\end{aligned}
$$

– .000,

$$- .000,003,701,894, \times \frac{1}{64}$$

$$- .000,001,139,044, \times \frac{1}{128}$$

$$- \&c. = .040,000,000,000, - .018,518,518,518,$$
$$- .000,638,569,604,$$
$$- .000,041,198,038,$$
$$- .000,003,745,276,$$
$$- .000,000,428,031,$$
$$- .000,000,057,842,$$
$$- .000,000,008,898,$$
$$- \&c.$$

$$= .040,000,000,000, - .019,202,526,207, - \&c.$$
$$= .020,797,473,793 - \&c.$$

Therefore the ſeries $\frac{1}{25} - \frac{x}{27} + \frac{xx}{29} - \frac{x^3}{31} + \frac{x^4}{33} - \frac{x^5}{35} + \frac{x^6}{37} - \frac{x^7}{39} +$ &c. or $\frac{1}{25} - \frac{tt}{27rr} + \frac{t^4}{29r^4} - \frac{t^6}{31r^6} + \frac{t^8}{33r^8} - \frac{t^{10}}{35r^{10}} + \frac{t^{12}}{37r^{12}} - \frac{t^{14}}{39r^{14}} +$ &c. is in this caſe $= .020,797,473,793$. Therefore the ſeries $\frac{t^{25}}{25r^{24}} - \frac{t^{27}}{27r^{26}} + \frac{t^{29}}{29r^{28}} - \frac{t^{31}}{31r^{30}} + \frac{t^{33}}{33r^{32}} - \frac{t^{35}}{35r^{34}} + \frac{t^{37}}{37r^{36}} - \frac{t^{39}}{39r^{38}} +$ &c. is in this caſe equal to $\frac{t^{25}}{r^{24}} \times .020,797,473,793$, that is, to $\frac{r^{25}}{r^{24}} \times .020,797,473,793$, or $r \times .020,797,473,793$; that is, the remainder of the infinite ſeries $t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} +$ &c. after the firſt twelve terms, is $= r \times .020,797,473,793$. But we before found

7 thoſe

thoſe firſt twelve terms to be $= r \times .764,600,691,483$. Therefore the whole ſeries $t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} +$ &c. *ad infinitum* is in this caſe $= r \times .764,600,691,483$, $+ r \times .020,797,473,793, = r \times .785,398,165,276$, which is true to eight places of figures, the more exact value of that ſeries being $r \times .785,398,163,397$, &c.; ſo that the value here found for this ſeries, by the help of only eight terms of the differential ſeries, differs from its true value by leſs than an unit in the eighth place of decimal figures, that is, by leſs than an hundred-millionth part of the radius r, which is a degree of exactneſs that could not have been attained by the mere computation of the ſeries $t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \frac{t^{11}}{11r^{10}} +$ &c. itſelf without computing fifty millions of its terms. There cannot be a ſtronger inſtance of the utility of that differential ſeries.

Computation of the ſeries which expreſſes the time of the deſcent of a pendulum through the arch of a circle.

Art. 16. As another example of the utility of the foregoing differential ſeries in finding the value of a ſeries that converges very ſlowly, I will now apply it to the ſeries which expreſſes the time of deſcent of a heavy body through a circular arch of 90°, which de-

creaſes

creaſes almoſt as ſlowly as the above-mentioned ſeries $r-\frac{r}{3}+\frac{r}{5}-\frac{r}{7}+\frac{r}{9}-\frac{r}{11}+\frac{r}{13}-\frac{r}{15}+$ &c., which expreſſes the magnitude of a circular arch of $45°$ in a circle whoſe radius is r.

Art. 17. If a heavy body, or a pendulum, be ſuppoſed to deſcend by the mere force of gravity through any arch of a circle not exceeding the arch of a quadrant, or $90°$; and the motion be ſuppoſed to begin from a ſtate of reſt, and to continue till the bob of the pendulum, or the heavy body, comes to the loweſt point of the circle; and the radius of the circle be called r, the perpendicular height, or verſed ſine, of the arch through which the deſcent is made, be called v, and the right ſine of the ſame arch be called s; and π be put for the number 1.570,796,326,794, &c. which expreſſes the ſemi-circumference of a circle whoſe diameter is called 1; and the time of the fall of a heavy body through the verſed ſine v, or the perpendicular altitude of the arch through which the pendulum deſcends, be denoted by v; the time of the deſcent of the pendulum through the ſaid circular arch, correſponding to the verſed ſine or altitude v, to the loweſt point of the circle, will be expreſſed by the product of $\pi\times\frac{rv}{s}$ into the ſeries

$$1-\frac{1.1}{2.2}\times\frac{vv}{ss}+\frac{1.1\,3.3}{2\,2.4.4}\times\frac{v^4}{s^4}-\frac{1.1.3.3.5.5}{2.2.4.4.6.6}\times\frac{v^6}{s^6}+\frac{1.1.3\,3.5.5.7.7}{2.2.4.4.6.6.8.8}\times\frac{v^8}{s^8}$$

$-\frac{1.1.3.3.5.5.7.7.9.9}{2.2.4.4.6.6.8.8.10.10} \times \frac{v^{10}}{s^{10}}$ + &c. in which the law of the continuation of the terms is very manifeſt, every new term being derived from the preceding term by multiplying it into the fraction $\frac{vv}{ss}$, and likewiſe into a numeral fraction, whoſe denominator is the ſquare of the index of the powers of v and s in the new term, and whoſe numerator is the ſquare of the odd number that is leſs than the ſaid index by an unit.

Art. 18. Let the numeral co-efficients of the terms of this ſeries be denoted by the capital letters of the alphabet, A, B, C, D, E, F, G, H, &c. in their natural order, ſo that A ſhall be equal to 1, and B ſhall be $= \frac{1.1}{2.2}$, and C $= \frac{1.1.3.3}{2.2.4.4}$, and D $= \frac{1.1.3.3.5.5}{2.2.4.4.6.6}$, and ſo on of the reſt. And we ſhall have

$B = \frac{1.1}{2.2}A$, $C = \frac{3.3}{4.4}B$, $D = \frac{5.5}{6.6}C$, $E = \frac{7.7}{8.8}D$, $F = \frac{9.9}{10.10}E$, $G = \frac{11.11}{12.12}F$, $H = \frac{13.13}{14.14}G$, and ſo on; and conſequently the ſeries

$$1 - \frac{1.1}{2.2} \times \frac{vv}{ss} + \frac{1.1.3.3}{2.2.4.4} \times \frac{v^4}{s^4} - \frac{1.1.3.3.5.5}{2.2.4.4.6.6} \times \frac{v^6}{s^6} + \&c.$$

or $A - \frac{Bvv}{ss} + \frac{Cv^4}{s^4} - \frac{Dv^6}{s^6} + \frac{Ev^8}{s^8} - \frac{Fv^{10}}{s^{10}} + \frac{Gv^{12}}{s^{12}} - \frac{Hv^{14}}{s^{14}}$ + &c. will be $A - \frac{1.1.Avv}{2.2.\ ss} + \frac{3.3.Bv^4}{4.4.\ s^4} - \frac{5.5.Cv^6}{6.6.\ s^6} + \frac{7.7\ Dv^8}{8.8.\ s^8} - \frac{9.9.\ Ev^{10}}{10.10.\ s^{10}} + \frac{11.11.Fv^{12}}{12.12.\ s^{12}} - \frac{13.13.Gv^{14}}{14.14.\ s^{14}}$ + &c. or $A - \frac{1Avv}{4ss} + \frac{9Bv^4}{16s^4} - \frac{25Cv^6}{36s^6} + \frac{49Dv^8}{64s^8} - \frac{81Ev^{10}}{100s^{10}} + \frac{121Fv^{12}}{144s^{12}} - \frac{169Gv^{14}}{196s^{14}}$ + &c. or, if we convert the co-efficients of the terms into decimal fractions,

1 –

$$1 - .250,000,000,000, \times \frac{vv}{ss} + .140,625,000,000, \times \frac{v^4}{s^4}$$
$$- .097,656,250,000, \times \frac{v^6}{s^6} + .074,768,066,406, \times \frac{v^8}{s^8}$$
$$- .060,562,133,788, \times \frac{v^{10}}{s^{10}} + .050,889,015,196, \times \frac{v^{12}}{s^{12}}$$
$- .043,878,793,714, \times \frac{v^{14}}{s^{14}} +$ &c. The co-efficients of theſe terms decreaſe ſo ſlowly (eſpecially after the firſt twelve or fourteen terms) that, when the verſed ſine v is very nearly equal to the right ſine s (as is the caſe when the arch through which the heavy body deſcends is nearly equal to 90°, or the arch of a whole quadrant of a circle) it would be neceſſary to compute a vaſt number of the terms of the ſeries in order to obtain its value exact to ſeven or eight places of figures; and, when v is quite equal to s (as is the caſe when the arch, through which the deſcent is made, is exactly equal to 90°) the computation of the value of the ſeries to that degree of exactneſs in that direct manner becomes wholly impracticable. But by the help of the differential ſeries abovementioned its value may be found, even in this caſe, to that degree of exactneſs without much difficulty; more eſpecially if we compute the firſt twelve terms of the ſeries in the common way, and then apply the differential ſeries to the inveſtigation of the remaining part of it in the ſame manner as in the laſt example. This we ſhall now proceed to do.

Art. 19. The co-efficients of the first twelve terms of the series $A - \frac{1Avv}{4ss} + \frac{9Bv^4}{16s^4} - \frac{25Cv^6}{36s^6} + \frac{49Dv^8}{64s^8} - \frac{81Ev^{10}}{100s^{10}} + \frac{121Fv^{12}}{144s^{12}} - \frac{169Gv^{14}}{196s^{14}} +$ &c. are as follows:

$$A = 1 = 1.000,000,000,000;$$

$$B = \frac{1}{4} = .250,000,000,000;$$

$$C = \frac{9B}{16} = .140,625,000,000;$$

$$D = \frac{25C}{36} = .097,656,250,000;$$

$$E = \frac{49D}{64} = .074,768,066,406;$$

$$F = \frac{81E}{100} = .060,562,133,788;$$

$$G = \frac{121F}{144} = .050,889,015,196;$$

$$H = \frac{169G}{196} = .043,878,793,714;$$

$$I = \frac{225H}{256} = .038,565,346,037;$$

$$K = \frac{289I}{324} = .034,399,336,434;$$

$$L = \frac{361K}{400} = .031,045,401,131;$$

$$M = \frac{441L}{484} = .028,287,235,328.$$

But when v is $= s$, as it is in the case of an arch of 90°, $\frac{vv}{ss}$ and all its powers will be $= 1$, and the twelve terms $A - \frac{Bvv}{ss} + \frac{Cv^4}{s^4} - \frac{Dv^6}{s^6} + \frac{Ev^8}{s^8} - \frac{Fv^{10}}{s^{10}} + \frac{Gv^{12}}{s^{12}} - \frac{Hv^{14}}{s^{14}} + \frac{Iv^{16}}{s^{16}} - \frac{Kv^{18}}{s^{18}} + \frac{Lv^{20}}{s^{20}} - \frac{Mv^{22}}{s^{22}}$

 will

will be equal to their co-efficients A−B+C−D+E−F+G−H+I−K+L−M. Therefore in this cafe the firft twelve terms of this feries are

1.000,000,000,000, −.250,000,000,000,
+ .140,625,000,000, −.097,656,250,000,
+ .074,768,066,406, −.060,562,133,788,
+ .050,889,015,196, −.043,878,793,714,
+ .038,565,346,037, −.034,399,336,434,
+ .031,045,401,131, −.028,287,235,328,

which are = 1.335,892,828,770, −.514,783,749,264,
= .821,109,079,506.

Art. 20. The remaining part of this feries is

$$\frac{Nv^{24}}{s^{24}} - \frac{Ov^{26}}{s^{20}} + \frac{Pv^{28}}{s^{28}} - \frac{Qv^{30}}{s^{30}} + \frac{Rv^{32}}{s^{32}} - \frac{Sv^{34}}{s^{34}} + \frac{Tv^{36}}{s^{36}} - \frac{Vv^{38}}{s^{38}} + \&c.$$

or $\frac{23.23.Mv^{24}}{24.24.s^{24}} - \frac{25.25.Nv^{26}}{26.26.s^{26}} + \frac{27.27.Ov^{28}}{28.28.s^{28}} - \frac{29.29.Pv^{30}}{30.30.s^{30}} + \frac{31.31.Qv^{32}}{32.32.s^{32}}$

$- \frac{33.33.Rv^{34}}{34.34.s^{34}} + \frac{35.35.Sv^{36}}{36.36.s^{36}} - \frac{37.37.Tv^{38}}{38.38.s^{38}} +$ &c. or

$.025,979,075,500, \times \frac{v^{24}}{s^{24}} - .024,019,115,661 \times \frac{v^{26}}{s^{20}}$

$+ .022,334,101,169, \times \frac{v^{28}}{s^{28}} - .020,869,976,759 \times \frac{v^{30}}{s^{30}}$

$+ .019,585,984,048, \times \frac{v^{32}}{s^{32}} - .018,450,810,232, \times \frac{v^{34}}{s^{34}}$

$+ .017,440,001,955, \times \frac{v^{36}}{s^{36}} - .016,534,184,679, \times \frac{v^{38}}{s^{38}}$

+ &c.; which is $= \frac{v^{24}}{s^{24}} \times$ the feries

$.025,979,075,500, - .024,019,115,661, \frac{vv}{ss}$

+ .022,

$+ .022,334,101,169, \frac{v^4}{s^4} - .020,869,976,759, \frac{v^6}{s^6}$

$+ .019,585,984,048, \frac{v^8}{s^8} - .018,450,810,232, \frac{v^{10}}{s^{10}}$

$+ .017,440,001,955, \frac{v^{12}}{s^{12}} - .016,534,184,679, \frac{v^{14}}{s^{14}}$

+ &c. or, if we fubftitute x in this laft feries inftead of $\frac{vv}{ss}$, $= \frac{v^{24}}{s^{24}} \times$ the feries

$$.025,979,075,500, - .024,019,115,661, x$$
$$+ .022,334,101,169, xx - .020,869,976,759, x^3$$
$$+ .019,585,984,048, x^4 - .018,450,810,232, x^5$$
$$+ .017,440,001,955, x^6 - .016,534,184,679, x^7$$

+ &c. Now the value of this laft feries may be difcovered by the application of the differential feries

$$a - \frac{bx}{1+x} - \frac{\text{D}^{\text{I}} xx}{\overline{1+x}|^2} - \frac{\text{D}^{\text{II}} x^3}{\overline{1+x}|^3} - \frac{\text{D}^{\text{III}} x^4}{\overline{1+x}|^4} - \frac{\text{D}^{\text{IV}} x^5}{\overline{1+x}|^5} - \frac{\text{D}^{\text{V}} x^6}{\overline{1+x}|^6} - \frac{\text{D}^{\text{VI}} x^7}{\overline{1+x}|^7} - \text{\&c.}$$

in the manner following:

Here a is $= .025,979,075,500$;
b $= .024,019,115,661$;
c $= .022,334,101,169$;
d $= .020,869,976,759$;
e $= .019,585,984,048$;
f $= .018,450,810,232$;
g $= .017,440,001,955$;
and h $= .016,534,184,679$.

Therefore

Therefore the differences of b, c, d, e, f, g, and h, of the several successive orders, are as follows:

First differences.

$b - c$ is $= .001,685,014,492$;
$c - d = .001,464,124,410$;
$d - e = .001,283,992,711$;
$e - f = .001,135,173,816$;
$f - g = .001,010,808,277$;
$g - h = .000,905,817,276$.

Second differences.

.000,220,890,082;
.000,180,131,699;
.000,148,818,895;
.000,124,365,539;
.000,104,991,001.

Third differences.

.000,040,758,383;
.000,031,312,804;
.000,024,453,356;
.000,019,374,538.

Fourth differences.

.000,009,445,579;
.000,006,859,448;
.000,005,078,818.

Fifth differences.

.000,002,586,131;
.000,001,780,630.

Sixth differences.

.000,000,805,501.

Therefore D^{I} is $= .001,685,014,492$;
$\text{D}^{\text{II}} = .000,220,890,082$;
$\text{D}^{\text{III}} = .000,040,758,383$;
$\text{D}^{\text{IV}} = .000,009,445,579$;
$\text{D}^{\text{V}} = .000,002,586,131$;
$\text{D}^{\text{VI}} = .000,000,805,501$.

Consequently the differential series

$$a - \frac{bx}{1+x} - \frac{\text{D}^{\text{I}}xx}{\overline{1+x}|^{2}} - \frac{\text{D}^{\text{II}}x^{3}}{\overline{1+x}|^{3}} - \frac{\text{D}^{\text{III}}x^{4}}{\overline{1+x}|^{4}} - \frac{\text{D}^{\text{IV}}x^{5}}{\overline{1+x}|^{5}} - \frac{\text{D}^{\text{V}}x^{6}}{\overline{1+x}|^{6}} - \frac{\text{D}^{\text{VI}}x^{7}}{\overline{1+x}|^{7}} - \text{\&c. is} = \text{to}$$

.025,

$$
\begin{aligned}
&.025,979,075,500,\\
&- .024,019,115,661, \times \frac{x}{1+x}\\
&- .001,685,014,492, \times \frac{xx}{\overline{1+x}|^2}\\
&- .000,220,890,082, \times \frac{x^3}{\overline{1+x}|^3}\\
&- .000,040,758,383, \times \frac{x^4}{\overline{1+x}|^4}\\
&- .000,009,445,579, \times \frac{x^5}{\overline{1+x}|^5}\\
&- .000,002,586,131, \times \frac{x^6}{\overline{1+x}|^6}\\
&- .000,000,805,501, \times \frac{x^7}{\overline{1+x}|^7}\\
&- \&c.
\end{aligned}
$$

This is the general value of the ſaid differential ſeries, whatever may be the value of x, or $\frac{vv}{ss}$. But in the caſe here ſuppoſed of an arch of 90°, the verſed ſine v is equal to the ſine s; and therefore $\frac{vv}{ss}$, or x, is $= 1$, and $\frac{x}{1+x} = \frac{1}{1+1}$, or $\frac{1}{2}$. Therefore the foregoing differential ſeries is in this caſe equal to

$$
\begin{aligned}
&.025,979,075,500,\\
&- .024,019,115,661, \times \frac{1}{2}\\
&- .001,685,014,492, \times \frac{1}{4}
\end{aligned}
$$

– .000,

$- .000,220,890,082, \times \frac{1}{8}$

$- .000,040,758,383, \times \frac{1}{16}$

$- .000,009,445,579, \times \frac{1}{32}$

$- .000,002,586,131, \times \frac{1}{64}$

$- .000,000,805,501, \times \frac{1}{128}$

$-$ &c. $= .025,979,075,500, - .012,009,557,830,$
$- .000,421,253,623,$
$- .000,027,611,260,$
$- .000,002,547,398,$
$- .000,000,295,174,$
$- .000,000,040,408,$
$- .000,000,006,292,$
$-$ &c.

$= .025,979,075,500, - .012,461,311,985,$ &c.
$= .013,517,763,515, -$ &c. Therefore the feries
$a - bx + cxx - dx^3 + ex^4 - fx^5 + gx^6 - bx^7 +$ &c., or
$.025,979,075,500 - .024,019,115,661, x$
$+ .022,334,101,169, xx - .020,869,976,759, x^3$
$+ .019,585,984,048, x^4 - .018,450,810,232, x^5$
$+ .017,440,001,955, x^6 - .016,534,184,679, x^7$
$+$ &c. is in this cafe $= .013,517,763,515, -$ &c.
Therefore $\frac{v^{24}}{s^{24}} \times$ this laft feries, or $\frac{v^{24}}{s^{24}} \times$ the feries

.025

$.025,979,075,500, - .024,019,115,661, \frac{vv}{ss}$

$+ .022,334,101,169, \frac{v^4}{s^4} - .020,869,976,759, \frac{v^6}{s^6}$

$+ .019,585,984,048, \frac{v^8}{s^8} - .018,450,810,232, \frac{v^{10}}{s^{10}}$

$+ .017,440,001,955, \frac{v^{12}}{s^{12}} - .016,534,184,679, \frac{v^{14}}{s^{14}}$

+ &c. is in this caſe $= \frac{v^{24}}{s^{24}} \times .013,517,763,515, -$ &c.: that is, the ſeries

$.025,979,075,500, \frac{v^{24}}{s^{24}} - .024,019,115,661, \frac{v^{26}}{s^{26}}$

$+ .022,334,101,169, \frac{v^{28}}{s^{28}} - .020,869,976,759, \frac{v^{30}}{s^{30}}$

$+ .019,585,984,048, \frac{v^{32}}{s^{32}} - .018,450,810,232, \times \frac{v^{34}}{s^{34}}$

$+ .017,440,001,955, \frac{v^{36}}{s^{36}} - .016,534,184,679, \frac{v^{38}}{s^{38}}$

+ &c., or $\frac{\text{N}v^{24}}{s^{24}} - \frac{\text{O}v^{26}}{s^{26}} + \frac{\text{P}v^{28}}{s^{28}} - \frac{\text{Q}v^{30}}{s^{30}} + \frac{\text{R}v^{32}}{s^{32}} - \frac{\text{S}v^{34}}{s^{34}} + \frac{\text{T}v^{36}}{s^{36}} - \frac{\text{V}v^{38}}{s^{38}}$ + &c. is in this caſe $= \frac{v^{24}}{s^{24}} \times .013,517,763,515, -$ &c. = (becauſe v is in this caſe $= s$, and conſequently $\frac{v^{24}}{s^{24}}$ is $= 1$) $.013,517,763,515, -$ &c. But we before found the value of the firſt twelve terms of the ſeries $\text{A} - \frac{\text{B}vv}{ss} + \frac{\text{C}v^4}{s^4} - \frac{\text{D}v^6}{s^6} + \frac{\text{E}v^8}{s^8} - \frac{\text{F}v^{10}}{s^{10}}$ + &c. to be in this caſe = $.821,109,079,506$. Therefore the value of the whole ſeries $\text{A} - \frac{\text{B}vv}{ss} + \frac{\text{C}v^4}{s^4} - \frac{\text{D}v^6}{s^6} + \frac{\text{E}v^8}{s^8} - \frac{\text{F}v^{10}}{s^{10}}$ + &c. *ad infinitum* is in this caſe $= .821,109,079,506, + .013,517,763,515,$

 − &c.

– &c. = 834,626,843,021, – &c. of which the first eight figures .834,626,84 are exact, the error being in the ninth figure 3, which ought to be a 2 instead of a 3, as would have appeared if we had computed another term or two of the differential series.

Art. 21. Since the series $A - \frac{Bvv}{ss} + \frac{Cv^4}{s^4} - \frac{Dv^6}{s^6} + \frac{Ev^8}{s^8} - \frac{Fv^{10}}{s^{10}}$ + &c., or $1 - \frac{1.1.Avv}{2.2.ss} + \frac{3.3.Bv^4}{4.4.s^4} - \frac{5.5\,Cv^6}{6.6.s^6} + \frac{7.7.Dv^8}{8.8.s^8} - \frac{9.9.Ev^{10}}{10.10.s^{10}}$ – &c. is, in this case of an arch of 90°, = .834,626,843, – &c., or somewhat less than .834,626,843, the product of that series into $\pi \times \frac{rv}{s}$, will be $= \pi \times \frac{rv}{s} \times$ .834,626,843, – &c. = (because v is in this case $= s$) $\pi \times r \times$ .834,626,843, – &c. = 1.570,796,326,794, &c. $\times r \times$ .834,626,843 – &c. = $r \times$ 1.311,028,779, – &c., or something less than $r \times$ 1.311,028,779; which is exact to nine places of figures, the more exact value of this quantity being $r \times$ 1.311,028,777,146, &c. as appears by a computation made by Mr. STIRLING, in his admirable Treatise on the Summation of Serieses, p. 58.

Art. 22. This value of the product of 1.570,796, 326,794, &c. $\times \frac{rv}{s}$ into the series

$1 - \frac{1.1\,Avv}{2.2.ss} + \frac{3.3.Bv^4}{4.4.s^4} - \frac{5.5.Cv^6}{6.6.s^6} + \frac{7.7\,Dv^8}{8.8.s^8} - \frac{9.9.Ev^{10}}{10.10.s^{10}}$ + &c., found by the foregoing processes in this extreme and most difficult case, to wit, 1.311,028,779, &c. $\times r$, exceeds its true

value,

value, 1.311,028,777,146, &c. $\times r$ by only .000,000, 002, $\times r$, or two thousand-millionth parts of the radius r; which is indeed a most minute difference, and shews the great exactness and utility of this differential series.

Art. 23. Of the nine figures to which the number 1.311,028,779, found by the foregoing process, is exact, the last eight are owing to the differential series. For if we were to multiply the value of the first twelve terms only of the series $A - \frac{Bvv}{ss} + \frac{Cv^4}{s^4} - \frac{Dv^6}{s^6} + \frac{Ev^8}{s^8} - \frac{Fv^{10}}{s^{10}} +$ &c, or $1 - \frac{1.1.Avv}{2.2.ss} + \frac{3.3.Bv^4}{4.4.s^4} - \frac{5.5.Cv^6}{6.6.s^6} + \frac{7.7.Dv^8}{8.8.s^8} - \frac{9.9.Ev^{10}}{10.10.s^{10}} +$ &c., to wit, the number .821,109,079,506, into $\pi \times r$, or 1.570,796, 326,794, &c. $\times r$, the product would be only 1.289, &c. $\times r$, which is true to only one place of figures, the second figure being a 2 instead of a 3. This therefore is an eminent proof of the utility of the said differential series.

Art. 24. In an arch of 90° the versed sine is equal to the radius of the circle, that is, according to the foregoing notation, v is $= r$. Therefore by art. 17. together with the foregoing computation, it appears, that the time of the descent of a pendulum, or other heavy body (moving freely from a state of rest by the force of gravity only) through the arch of a whole quadrant of a circle is to the time of the fall through the cor-

 respondent

refpondent perpendicular altitude, or the radius, as 1.311,028,779, – &c. $\times r$ is to r, or as 1.311,028,779, – &c. is to 1.

Art. 25. Hence we may determine the proportion of the time of defcent of a pendulum through an arch of 90° to the time of its defcent through an infinitely fmall arch at the bottom of a quadrant, or rather (to fpeak correctly) to the limit of the time of defcent through a very fmall but finite arch at the bottom of the quadrant, to which the faid time continually approaches nearer and nearer as the faid fmall arch is taken lefs and lefs, and to which it may be made to approach fo nearly, by taking the faid fmall arch fufficiently fmall, as to differ from it by lefs than any given quantity. For this latter time, or limit, is known to be to the time of the fall of a heavy body through half the length of the pendulum, or half the radius of the circle, as the femi-circumference of a circle is to its diameter, that is, as the number 1.570,796,326,794, &c. is to 1. But the time of the fall of a heavy body through half the radius of the circle is to the time of the fall through the whole radius as 1 to $\sqrt{2}$, or 1.414,213, &c. Therefore, *ex æquo*, the faid limit of the time of defcent of a pendulum through a very fmall arch of the circle at the bottom of the quadrant, is to the time of the fall of a heavy body through

the radius of the circle, or the whole length of the pendulum, as 1.570,796, &c. is to 1.414,213, &c. But we have feen in the laft article that the time of the fall of a heavy body through the radius of the circle is to the time of defcent of a pendulum through the arch of a whole quadrant as 1 to 1.311,028,779, - &c. Therefore the limit of the time of defcent of a pendulum through a very fmall arch at the bottom of the quadrant is to the time of defcent through the arch of the whole quadrant as 1.570,796,326, &c. × 1 is to 1.414,213, &c. × 1.311,028,779, - &c., or as 1.570,796,326, &c. is to 1.414,213, &c. × 1.311,028,779, - &c., that is, by art. 21. as 1.570,796,326, &c. is to 1.414,213, &c. × 1.570,796,326, &c. × .834,626,843, - &c., or as 1 to 1.414,213, &c. × .834,626,843, - &c., or as 1 to 1.180,340, &c., or, in fmaller numbers, as 1 to 1.180, or as 1000 to 1180, or as 100 to 118, or as 50 to 59.

Art. 26. This proportion of the times of the defcent of a pendulum through an infinitely fmall arch at the bottom of the quadrant, and through the arch of the whole quadrant, agrees pretty nearly with that affigned for them by Mr. HUYGENS in the preface to his admirable Treatife on Pendulum-clocks, or *De Horologio Ofcillatorio*, which is that of 29 to 34. For 50 is to 59 as 29 is to 34.2, or $34\frac{1}{5}$; or (neglecting the fraction $\frac{1}{5}$) as 29 is to 34; Mr. HUYGENS meaning

meaning, probably, in that place, not to expreſs this proportion as accurately as he could, but only as nearly as it could be expreſſed by ſmall whole numbers. However, the numbers 50 and 59 expreſs this proportion rather more accurately than 29 and 34, and with pretty much the ſame degree of ſimplicity, and therefore, upon the whole, are ſomewhat to be preferred to them.

Art. 27. I have endeavoured to find another differential ſeries, ſimilar to that above deſcribed, for the purpoſe of inveſtigating the value of an infinite ſeries of this form, to wit, $a+bx+cxx+dx^3+ex^4+fx^5+gx^6+hx^7+$ &c. (in which all the terms are marked with the ſign $+$, or are added to the firſt term a) when the co-efficients b, c, d, e, f, g, h, &c. decreaſe very ſlowly, and x is very nearly equal to 1, and the terms of the ſeries decreaſe conſequently ſo ſlowly as to make the ſummation of it in the common way, or by the mere computation and addition of its terms, almoſt impracticable; but my endeavours have not been attended with ſucceſs. I may therefore, from my own experience, ſubſcribe to the truth of what is aſſerted upon this ſubject by the very learned and ingenious Mr. JAMES STIRLING in his Treatiſe, intitled, *Summatio Serierum*, p. 17. to wit, that *Series quarum termini ſunt per vices negativi et affirmativi, ſunt magis tractabiles quam alteræ, ubi de Summatione agitur*; though at firſt ſight one would be apt to imagine the reverſe of this propoſition to be true.

XI. *Tranſlation of a Paſſage in* Ebn Younes; *with ſome Remarks thereon: in a Letter from the Rev.* George Coſtard, *M. A. Vicar of* Twickenham, *to the Rev.* Samuel Horſley, *LL.D. Sec. R. S.*

REV. SIR,

Twickenham, Jan. 9, 1777.

Read Feb. 13, 1777.

HAVING, by means of the Royal Society, been favoured with a tranſcript of the Arabic paſſage in manuſcript of EBN YOUNES, in the library at Leyden, I now ſend you as exact a tranſlation of it as I can. I give it you in Latin, as the former tranſlations of it were in that language; and as the numbers in the manuſcript by no means agree with calculations made by modern tables, I have ventured to ſuppoſe that they have been ſomehow or other altered from what they were in the original tables of EBN YOUNES. I have likewiſe ventured to ſuppoſe that the preſent Leyden copy is a tranſcript of another copy, which is no very violent ſuppoſition, conſidering how long ago theſe obſervations have been made, and how long it is ſince EBN YOUNES wrote.

I have likewise made no scruple to suppose that, however distinct and elegant both the Arabic letters and figures are in later manuscripts, they were not so in those of a more ancient date, so that the one might easily be mistaken for the other, where there is a similarity: and this mistake would be the more easily committed by a person ignorant of the subject he was upon. This probably was the case of all such as were hired by booksellers to transcribe manuscripts for sale; and for this reason, when the transcriber had made any mistake, he would not blot it out for fear of spoiling the sale of his book.

There is an instance of this sort in this very manuscript in the observations of the third eclipse, which is that of the Moon, as you will see in the transcript and translation sent you last year by Mr. SCHULTENS.

If what hath been said be allowed me, as I hope it will not be thought too much, I think I shall be able to account possibly, if not probably, for the differences between the observations as set down in the manuscript, and the result of the calculations by modern tables: a thing which hath not been hitherto attempted, as few who have been versed in astronomy have been acquainted with the Arabic language; and they on the other hand, who have well understood Arabic, have been as little conversant with astronomy.

What

What I have now advanced ſhall be exemplified under the firſt eclipſe, which is one of the Sun.

In this eclipſe, according to the manuſcript, at the beginning, the Sun's altitude was more than 15 (يه) degrees, and leſs than 16 (يو); and at the end it was more than 33 degrees (لج) and $\frac{1}{3}$. But I make the Sun's height at the beginning 30 (ل) degrees, and at the end nearly 36 (لو). In the manuſcript, the digits eclipſed are ſaid to have been 8 (٧, or ٨, as it is ſometimes written); but I make them only a little more than 4(د), or about $4\frac{4}{5}$.

Whether the notation in the original manuſcript of EBN YOUNES was in letters or arithmetical figures is uncertain; but moſt probably it was in the former of theſe two, as it is in moſt of the tables now extant, though compoſed ſince the admiſſion and uſe of arithmetical figures. Upon this ſuppoſition then, or that they were ſo in the manuſcript from whence the preſent manuſcript was copied, we ſhall very naturally account for the miſtakes we find in it.

Thus for inſtance, ل by ſome accidental ſtroke at the bottom, would eaſily be taken for له, as يه is ſometimes written in manuſcripts; and if the perpendicular ſtroke in the ل was made ſhort, as in a table it very well might be, ل (30) would naturally be taken for يه or يه (15); and,

 by

by the ſame rule, لو (36) would very eaſily be taken for يو (16); and د (4) the digits eclipſed for ٧ which is 8 in the other form of notation, or ح in this.

In the manuſcript it is ſaid, that the Sun's altitude at the end, by obſervation, was a little more than 33 (لج) degrees; but this would, in a manuſcript ill writen, eaſily be miſtaken for له (35) or لو (36).

As to the words, tranſlated by Profeſſor SCHULTENS for Mr. GRISCHOW, "accidit hoc in plano circuli ejus "minus quam 7 digiti," I am apt to ſuſpect they are nothing more than ſome marginal reading crept into the text; that is, ſomebody ſeeing the digits eclipſed here made 8 (ح), added, as the Arabic will very well bear, "imo minus quam ز (7) or ٧ (7)," as in the other form of notation that figure is ſometimes made. The writer of this manuſcript, whoever he was, was certainly acquainted with both forms of notation, as he hath made uſe of both.

This interpretation is at leaſt plauſible, and clears up a ſentence which greatly perplexed both Mr. GRISCHOW and Dr. BEVIS, and ſeemed to them quite unintelligible.

The account given by CURTIUS of the ſecond eclipſe, which was a ſolar one, is this:

Anno eodem, die Sabbati, videlicet, 29 mensis Sywal, (numero decimi, qui Paschalis est eorum) eclipsis Solis occupavit digitos $7\frac{1}{2}$. In principio, Sol altus ferè 56°. In fine, Sol occiduus elevabatur gradibus 26. Ex SHICKARDO in MS.

This it is plain is not a translation of the Arabic, for that, as translated by SCHULTENS for Mr. GRISCHOW, and transmitted by him to Dr. BEVIS, is much fuller, and is as follows:

ECLIPSIS SOLARIS.

Hæc eclipsis extitit die Sabbati, 29 mensis Siewal, anno 367 Hegiræ. Et dies Sabbati hicce ipse est dies 9 mensis Chordadma, anni 348 Jesdagirdis, et ipse 8 mensis Haziran anni 1289 Alexandri, et ipse est 14 mensis Buna, anni Dioclesiani.

Fuitque maximum quod eclipsatum est de diametro Solis, 5 digiti et $\frac{1}{2}$ super calculo accuratiore.

Erantque de plano circuli ejus 4 digiti et 10 minuta.

Et erat elevatio Solis, tempore quo eclipsis incepit, secundum oculum 56° circiter; et erat integra ejus reapparitio cum esset elevatio ejus 26 graduum, aut circiter; erantque Sol et Luna simul in hac eclipsi, in pro-

pinquo diftantiæ maximæ a terrâ. Deus fcit an calculus hic benè fit pofitus. Tempus refpondet diei 8 Jun. an. Chrifti 978.

Thus far Mr. SCHULTENS. And here I muft obferve that, according to him as well as CURTIUS, the Sun's altitude at the beginning was about 56°, or in Arabic notation نو; but by computation I make it only about 47° 50′. Suppofe it were 47 (مز); then where the letters are fmall and ill made, نو and مز may eafily be miftaken for each other.

The Sun's altitude at the end of this eclipfe, according to both CURTIUS and SCHULTENS, was 26° (كو) ; but by calculation I make it a little more than 36° (لو). But thefe figures are fo nearly alike that they would eafily be miftaken by an ignorant tranfcriber, and from a manufcript that was ill wrote.

How SCHICKARD, or CURTIUS for him, came to make the digits eclipfed $7\frac{1}{2}$ I know not: for in the manufcript, as tranflated by SCHULTENS for Mr. GRISCHOW above, we fee they were only $5\frac{1}{2}$ and that *fuper calculo accuratiore*, or as the Arabic fhould have been tranflated, *juxta calculum accuratiorem*. The meaning of which, I fuppofe, is that EBN YOUNES had found by calculation that the digits eclipfed would be $5\frac{1}{2}$, and that at the time his calculation agreed with his obfervation; as indeed it did,

for

for I make them about $5\frac{3}{5}$, however widely this differs from $7\frac{1}{2}$ as in CURTIUS.

When the altitude of the Sun, at the beginning of this eclipſe, is ſaid to have been 56° or nearly, *ſecundum oculum*, it is evident that this was an obſervation.

When it is added, *erantque de plano circuli ejus* 4 *digiti et* 10 *minuta*, in words at length, it ſeems to have been ſome interpolation or marginal reading, crept into the text, as another ſeems to have done under the former eclipſe; for if the digits eclipſed here were $5\frac{1}{2}$, agreeable both to obſervation and *accurate calculation*, they muſt certainly have been more than 4° 10′.

At the concluſion of the former eclipſe it was added in the tranſlation, Deus ſcit an *obſervatio* ſit benè inſtituta; and here the paſſage, as tranſlated, concludes with Deus ſcit an *calculus* hic benè ſit poſitus. But in the Arabic, as I have received it, there is no mention made either of *obſervation* or *calculation*. The words are the ſame in both paſſages, and are only *adjuvante Deo*. The other tranſlations ſeem only to have been what Mr. GRISCHOW collected from profeſſor SCHULTENS, who, he ſays, was totally ignorant of aſtronomical language, as he himſelf was ignorant of Arabic.

The third is a Lunar eclipſe; and the account given of it by CURTIUS, from SCHICKARD, is this:

Anno

Anno Chrifti 979. Anno Hegiræ 368 (qui incepit d. 8 Aug. mihi die 9 Aug. anno Chriftiano 978) die Jovis, 14 Sywal, Luna fuit orta cum defectu, qui ad $5\frac{1}{2}$ digitos accrevit, cum extaret fupra horizontem gradibus etiam 26 (fubaudio finem tunc accidiffe). SCHICKARDUS. Qui adjungit, tempus refpondere diei 14 Maii, anno Chrifti 979.

The account of this eclipfe, as tranflated by Profeffor SCHULTENS for Mr. GRISCHOW, is more particular and intelligible.

Eclipfis Lunæ extitit in menfe Sieval (five Xaval) anno 368 Hegiræ. Orta eft Luna eclipfata, in nocte cujus aurora fuit feria quinta. Et hæc feria quinta fuit dies 25 menfis Ijar, anni 1290 Alexandri, et ille 20 menfis Bafchner (five Pachon) anni 695 Dioclefiani.

Spatium quod eclipfatum fuit de diametro ejus, fuit amplius quam octo digiti, et minus quam novem.

Fuitque hora ortûs ejus proxima horæ oppofitionis, fecundum fundamenta quibus computare foleo.

Et perfecta eft ejus reapparitio (five finis) cum præteriiffet de nocte (i. e. poft occafum Solis) circiter hora jufta, et quinta horæ pars, prout obfervavi. Et erat Luna, in hac eclipfi, in propinquo diftantiæ fuæ mediæ.

Tempus refpondet diei 14 Maii, anno Chrifti 979.

With

With regard to the time of the opposition, and the Moon's rising at Cairo, there is very little difficulty; for she rose there at $6^h\ 48'\ 10''$, and the time of opposition was at $6^h\ 24'\ 36''$.

The end of this eclipse there was at $7^h\ 54'\ 26''$, and the time of Sun-set was at $6^h\ 47'\ 52''$. The difference is $1^h\ 6'\ 16''$, and agrees very well with the manuscript.

The passage, as we have it here in CURTIUS from SCHICKARD, is very obscure. For it seems either to mean that when the digits eclipsed were $5\frac{1}{2}$ the Moon was $26°$ high, or that she was $26°$ high when the eclipse ended. But I take the last to be intended; for the Moon was $26°$ high at $7^h\ 36'$, and the eclipse ended, as we saw, at $7^h\ 54'\ 26''$.

But when SCHICKARD or CURTIUS say this *defectus ad $5\frac{1}{2}$ digitos accrevit*, the meaning must be that they amounted only to $5\frac{1}{2}$. But this is not true; for according to the manuscript, they were between 8 and 9, and I make them about $8\frac{3}{5}$.

I am apt to suspect, therefore, that the transcriber, whoever he was, cast his eyes on the Solar eclipse above, where the digits eclipsed are really $5\frac{1}{2}$, and carelessly set them down to this Lunar eclipse where they do not belong. And to confirm this conjecture it must be observed, that after the word *Dioclesian* under this Lunar eclipse, in the Arabic follow *six lines*, which are a repetition

tition of all that was ſaid under the laſt ſolar eclipſe, from the ſame word *Diocleſian* to the end of that obſervation.

I ſhall now, in the laſt place, give you a tranſlation of the Arabic paſſage intire, omitting however the interpolations mentioned above, which embarraſs the whole.

Infit ALI IBN ABDORRAHMAN, IBN ACHMED, IBN YOUNES, IBN ABDOL' AALI.

Imprimis, jam commemoravi eclipſes, tam Solares quam Lunares, quas obſervârunt viri docti; eruditi ii quorum nomina recenſui, quaſque ad eos retuli, *incipiendo* ab auctoribus libri dicti ALMOMTAHEN, uſque ad filios Majour; quin et conjunctiones eorum cum ſtellis fixis, quas obſervârunt, et quorum loca commemorârunt, et invenerunt, tempore conjunctionum eorum.

Ipſe deinde memorabo eclipſes quas obſervavi, tam Solares quam Lunares, et conjunctiones cum ſtellis fixis, et quænam fuerunt formæ eorum in conjunctionibus ſuis. Ut quicunque me ſequantur, et indicia habere deſiderent, meis utantur, quemadmodum ac ego eorum indiciis et directionibus uſus ſum, qui ante me obſervârunt. Deus autem adjutor eſt.

Eclipſis Solaris erat priore parte diei, feriâ quintâ, die decimo octavo menſis Rabiæ poſterioris, anno Hegiræ

367.

367. Et hæc feria quinta erat dies decimus ſecundus menſis Adzermah, anno Yezdagerdis 346.

Caraffæ adfuimus, in templo ABI GAAFARI ACHMED IBN NASAR Africani, cœtus eruditorum, ad hanc elipſin obſervandam. E quorum numero erat HAROUN IBN MOHAMMED AL GAAFARI, et ABU ABDALLAH AL HOSEIN IBN NASAR Africanus, et ABUL' HOSEIN ALI IBN MAHARBACHT Perſa, et ABUL' ABAS ACHMED IBN ACHMED AL CHURGII, et ABU ACHMED ASSUMACHI, et ABU OMAR Scriba.

Ex his, præter alios eruditos cum reliquis obſervatoribus, nonnulli erant aſtronomicè docti.

Ipſe quoque eodem contendi, unâ cum ABUL' KASEM ABDORRAHMAN IBN HOSEIN, IBN TISAN, AL IDAS, et HOSAN IBN AL DARANI, et HAMED IBN AL HOSEIN.

Et hi omnes initium hujus eclipſeos obſervârunt, quæ, ad ſenſum meum, apparere incepit ſole plus quam gradibus 15, minus autem quam 16 elevato.

Omnes quoque præſentes opinione conſentierunt obſcurari de diametro ejus circiter 8 digitos.

* * * * * *

Et ſplendor ejus perfectè recuperatus eſt cum elevaretur amplius quam gradibus 33 cum tertiâ ferè parte, prout ipſe menſuravi; omnibus qui aderant conſentientibus.

In hac eclipsi, Sol et Luna simul erant non longè a distantiâ suâ proximâ a terrâ. Adjuvante Deo.

ECLIPSIS SOLARIS.

Hæc eclipsis incidit in diem Sabbati, diem 29 mensis Shuwal, anno Hegiræ 367. Eratque hic dies Sabbati, dies 9 mensis Chordadmah, anno Yezdagerdis 347, et dies 8 mensis Haziràn, anno Alexandri 1289; et insuper dies 14 mensis Bounah. anno Dioclesiani 694.

Maximum quod obscuratum est de diametro solis erat $5\frac{1}{2}$ digiti.

* * * * * *

Et quando hæc eclipsis, ad oculi aciem, jam incepisse constabat, Solis altitudo erat circiter gradus 56, et lucis ejus restitutio completa est cum altitudo ejus esset 26 gradus, vel circiter.

Erantque Sol et Luna simul, in hac eclipsi, propè distantias suas maximas a terrâ. Adjuvante Deo.

ECLIPSIS LUNARIS.

Hæc contigit mense Shuwal, anno Hegiræ 368. Oriebatur Luna, eclipsi jam inchoatâ, nocte cujus Aurora erat feria quinta, quæ feria quinta erat dies 28 mensis Ardbahest, anno Yezdagerdis 348, quæ fuit 18 mensis

Ijar

Ijar, anno æræ Alexandri 1290. Eratque dies 20 menſis Biſhnis, anno Diocleſiani 698.

* * * * * *

Eratque quantitas diametri ejus obſcurata, pluſquam digiti 8, et minus quam novem. Tempuſque ortûs ejus erat propè tempus oppoſitionis, juxta fundamenta quibus computavi: lucemque plenam recuperavit cum de nocte præteriiſſet hora circiter æquinoctialis, cum quintâ parte, prout ipſe conjectavi.

Eratque Luna in hac eclipſi, haud procul a diſtantiâ ſuâ mediâ a terrâ. Adjuvante Deo.

This I hope will be ſufficiently ſatisfactory.

I am, &c.

XIII. *Obfervations on the Annual Evaporation at* Liverpool *in* Lancafhire; *and on Evaporation confidered as a Teft of the Moifture or Drynefs of the Atmofphere. By Dr.* Dobfon *of* Liverpool. *Communicated by John* Fothergill, *M. D. F. R. S.*

Read Feb. 13, 1777.

THE quantity of rain which falls during the courfe of the year, is a very uncertain teft of the moifture or drynefs of any particular feafon, fituation, or climate. There may be little or even no rain, and yet the air be conftantly damp and foggy; or there may be heavy rains, with a comparatively dry ftate of the atmofphere. The fame depth of rain will likewife produce different effects on the air, according as it falls upon a flat or hilly country; for large quantities foon quit the hills or high grounds, while fmaller quantities have more lafting and powerful effects on a flat country. Much alfo depends upon the nature of the foil, whether clay or fand, whether firm and compact, or loofe and fpungy.

Is not evaporation therefore a more accurate teft of the moifture or drynefs of the atmofphere, than the quantity of rain?

It is well known, that air is an active ſolvent of water, and that its powers of ſolution are in proportion to its dryneſs. It is likewiſe well known, that in chemical ſolutions, the action of the *menſtruum* is greatly promoted by heat and agitation. If the temperature of the air then, and the ſtate of the winds, be aſcertained, which in the preſent caſe denote the heat and agitation of the *menſtruum*, the evaporation will be the true index of the dryneſs of any particular ſeaſon, ſituation, or climate.

To determine the annual evaporation in the neighbourhood of Liverpool, I procured two well-varniſhed tin veſſels; one of which was to ſerve the purpoſe of a rain-gage; the other was to be employed as my evaporating veſſel. The evaporating veſſel was cylindrical, twelve inches in diameter and ſix inches deep. The rain-gage conſiſted of a funnel twelve inches likewiſe in diameter, the lower end of which was received into the mouth of a large ſtone-bottle; and, to prevent any evaporation from the bottle, the pipe of the funnel was ſtopped with a grooved cork. Theſe veſſels were placed in the middle of a graſs-plot, on a riſing ground adjoining and immediately overlooking the town, about ſeventy-five feet above the level of the ſea, and with a free expoſure to the Sun, winds, and rain. The cylindrical veſſel was filled with water within two inches of the

top;

top; and if, in conſequence of heavy rains, there was danger of its overflowing, a quantity of water was taken out; but if, in conſequence of long drought, it ſunk lower, a quantity of water was then occaſionally added; and theſe additions or ſubtractions were carefully regiſtered. At the end of every month, the depth of rain was firſt calculated; and, as each veſſel received the ſame depth of rain, I had only to examine the quantity of water which had been added to, or taken out of, the evaporating veſſel, and the evaporation of the month was aſcertained.

The firſt column of the following tables points out the mean temperature of the air at two in the afternoon. The ſecond, the character of the month with reſpect to the winds, the number of dots expreſſing their ſtrength; and, to make this part tolerably accurate, daily obſervations on the winds were marked down, and the character of the month formed from a general ſurvey of theſe obſervations: our winds are Weſterly for near two-thirds of the year. The third column points out the evaporation of each month in inches and decimal parts of an inch. The fourth, the depth of rain during each month. And the fifth, the ſtate of the ſeaſons, E being prefixed to the evaporation of the whole three months, R to the rain, and T to the mean temperature.

It is to be obferved, that in making thefe experiments, 251 grains were allowed for every cubic inch of water; and that three pounds and twelve ounces of water give a depth of one inch on a circular area of twelve inches diameter.

TABLE I.

A comparative view of the evaporation, rain, winds, and temperature of the air, during the year 1772.

Months.	Temp.	Winds.	Evaporat.	Rain.	Seafons.
January	38	. . .	1.27	3.26	E. 4.87
February	39	. . . .	1.25	2.35	R. 7.23
March	44	. . .	2.35	1.62	T. 40.
April	48	. .	2.53	1.85	E. 11.40
May	57	.	4.25	3.42	R. 8.39
June	67	. .	4 62	3.12	T. 57.
July	70	. .	5.53	1.59	E. 13.20
Auguft	68	. .	5.35	3.65	R. 11.29
September	62	.	2.32	6.05	T. 66.
October	60	. .	3.18	3.42	E. 6.46
November	50	. . .	2.15	4.85	R. 10.48
December	44	. .	1.13	2.21	T. 51.
	54		35.95	37.39	

TABLE

TABLE II.

A comparative view of the evaporation, rain, winds, and temperature of the air, during the year 1773.

	Temp.	Winds.	Evaporat.	Rain.	Seasons.
January	44	. . . .	1.85	3.15	E. 5.74
February	42½	. . .	1.13	2.37	R. 6.17
March	50	. . .	2.76	0.65	T. 45.
April	54	. . .	2 89	2.47	E. 9.34
May	57	. .	3.79	4.56	R. 8 35
June	64½	.	2.66	1.42	T. 58.
July	67	. .	4.92	1.32	E. 14.02
August	70	.	5.75	2.21	R. 10.08
September	60	. .	3.35	6.55	T. 65.
October	55	. . .	2.79	4.57	E. 5.49
November	47½	. . .	1.15	6.69	R. 15.58
December	41½	. . .	1.55	4.32	T. 48.
	54½		34.59	40.18	

TABLE III.

A comparative view of the evaporation, rain, winds, and temperature of the air, during the year 1774.

	Temp.	Winds.	Evaporat.	Rain.	Seasons.
January	37	. . .	1.38	4.43	E. 5 92
February	$45\frac{1}{3}$	. . . .	1.67	2.42	R. 8.23
March	$49\frac{2}{3}$	. . .	2.87	1.38	T. 44.
April	$54\frac{1}{2}$	. . . .	4.56	2 23	E. 12.39
May	$59\frac{1}{2}$	. . .	4.31	1.65	R. 7.14
June	63	. .	3.52	3.26	T. 59.
July	$66\frac{2}{3}$	. . .	4.97	2.68	E. 13.51
Auguft	67	. .	4.52	2.36	R. 10.56
September	$61\frac{1}{3}$	. .	4.02	5.52	T. 65.
Octobr	57	. .	1.95	1.68	E. 4.82
November	$46\frac{1}{3}$	. . .	1.12	2.69	R. 6.00
December	$41\frac{3}{4}$	. .	1.75	1.63	T. $48\frac{1}{3}$.
	54		36 64	31.93	

TABLE IV.

A comparative view of the evaporation, rain, winds, and temperature of the air, during the year 1775.

	Temp.	Winds.	Evaporat.	Rain.	Seasons.
January	44½	. . .	1.51	3.21	E. 7.10
February	49	. . . .	3.02	4.62	R. 10.28
March	48½	. . .	2.57	2.45	T. 47⅓.
April	57⅝	. . .	3.21	1.01	E. 15.09
May	61	. . .	5.02	0.85	R. 3.98
June	70½	. . .	6.86	2.12	T. 63.
July	68½	. .	5.03	5.31	E. 12.50
August	66½	. .	4.42	4.26	R. 12.57
September	65	. .	3.05	4 00	T. 56⅓.
October	54½	. . .	2.12	7.01	E. 5.27
November	45	. .	1.63	3.03	R. 13.39
December	48⅓	. .	1.52	3.35	T. 49⅓.
	54		39.96	40.22	

OBSERVATIONS.

1. It is evident from these tables, whether we attend to separate months, seasons, or years, that the depth of rain is a very erroneous index of the moisture or dryness of the atmosphere. On comparing the two months July and August of the year 1772, it appears that the tem-

 perature

perature of the air, the ſtate of the winds, and the evaporation, were nearly the ſame during theſe two months, and yet the rain of Auguſt was more than double that of July. The reaſon why the greater quantity of rain had no more effect than the ſmaller in adding moiſture to the atmoſphere, is obvious; for on conſulting my regiſter I find, that the rain of Auguſt fell in heavy ſhowers, and ran off the ground before it could be evaporated; while that of July, falling in ſmall drizzling ſhowers, gave more time for its evaporation.

Again, the temperature of the air, the ſtate of the winds, and the evaporation, were nearly the ſame during the firſt three months of the year 1773, with what they were during the laſt three months of that year; the ſtate of the air therefore, with reſpect to moiſture and dryneſs, muſt have been the ſame during theſe two ſeaſons; and yet the depth of rain, in one of theſe ſeaſons, was much more than double what it was in the other. If we attend to whole years the ſame obſervation is confirmed. The rain of 1775 exceeded the rain of 1774 more than eight inches; and hence it might be concluded, that the atmoſphere was more moiſt in 1775 than in 1774; the reverſe of this, however, is found to be the fact: for there evaporated from a conſtant and determinate ſurface of water in 1775, full three inches more

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than evaporated from the ſame ſurface of water in 1774. Conſequently the dryneſs of the atmoſphere or its power of ſolution, during the year 1775, exceeded that of 1774.

2. If we take the medium of four years obſervations it appears, that the annual evaporation at Liverpool amounts to 36.78 inches.

Dr. HALLEY obſerved at London, that water placed in a cloſe room, where neither the winds or Sun could act upon it, exhaled only eight inches during the whole year. He makes no doubt but that the free acceſs of the winds would have trebled the quantity carried away; and that this again would have been doubled by the aſſiſtance of the Sun. Dr. HALLEY, therefore, fixes the annual evaporation of London at 48 inches [a]. If this calculation be admitted, it follows, that the annual evaporation of London exceeds the annual evaporation of Liverpool 11 inches; but were the experiments to be made in London, in the ſame circumſtances with thoſe made at Liverpool, it is probable, that this would be found to be more than the real difference.

The learned CRUQUIUS obſerved at Delft in Holland, that there exhaled from water ſet in the open air, but in a calm and ſhady place, about 30 inches; and it is not to

(a) Phil. Tranſ. N° 212.

be

be doubted, ſays Dr. BROWNRIGG in his very valuable work, *The Art of making common Salt*, but that double this quantity, or 60 inches, would have exhaled, had it been placed where the Sun and winds could have had their due effects [(b)]. In another part of this publication, Dr. BROWNRIGG fixes the evaporation of ſome parts of England at 73.8 inches during the four ſummer months, May, June, July, and Auguſt; and the evaporation of the whole year at upwards of 140 inches [(c)]. Theſe are calculations, however, which do not appear to correſpond with experience; for the whole evaporation at Liverpool, inſtead of 140 inches, was only 36.78 inches. The evaporation likewiſe of the four ſummer months, on a medium of four years, inſtead of 73 inches, was only 18.88 inches.

3. Dr. HALES calculates the greateſt annual evaporation from the ſurface of the earth in England, even that from a ſurface of hop-ground, at 6.66 inches [(d)]. If we compare this with the annual evaporation from a ſurface of water as determined by experiment, we find, that the latter exceeds the former about 30 inches; and that the annual evaporation from a ſurface of water, is to the annual evaporation from the ſurface of the earth in this part of England, nearly as 36 to 6, or as 6 to 1.

(*b*) Page 185. (*c*) P. 189.
(*d*) Veg. Stat. vol. I. p. 55, 56.

4. On comparing the depth of rain with the annual evaporation of this part of Lancaſhire we find, that more falls in rain than is raiſed in vapour, even though the whole were a ſurface of water; for the rain is to the evaporation as 37.43 inches to 36.78 inches: and we farther find, that the quantity exhaled from the ſurface of the earth is little more than a ſixth part of what deſcends in rain; we muſt therefore have very large ſupplies from other regions, from the ſurrounding ſea, and from the ocean of warmer climates. Hence we ſee, why our South and South-weſt winds are ſo often accompanied with rain; for as the air ſweeps along the warmer latitudes, it involves a large proportion of moiſture, which is conſtantly and copiouſly exhaling from the ocean; and this moiſture being retained in a ſtate of ſolution till it reaches the colder climates, is then either collected in clouds or immediately precipitated in rain, according to the different conditions of the atmoſphere.

Theſe foreign ſupplies, however, are uniformly reſtored to the ſources from which they were derived: for that proportion of rain which riſes not in vapour, after moiſtening and refreſhing the earth, forms ſprings, brooks, and rivers, and is thus perpetually returning to the ocean whence it was taken; ſo truly philoſophical are the words of the preacher when ſpeaking of this vaſt circulation:

circulation: "All the rivers run into the ſea, yet the ſea "is not full: unto the place from whence the rivers "*come*, thither they return again."

5. About a century ago, the ingenious Mr. TOWNLEY, of Townley in this county, made ſome accurate obſervations on the depth of rain which fell annually in the neighbourhood of the hills which divide Lancaſhire and Yorkſhire; and on taking a medium of fifteen years, he determines it to be 41.516 inches[(e)]. The depth of rain, therefore, at Townley exceeds the depth of rain at Liverpool about four inches. This is probably, however, leſs than the real difference; for there was a ſource of error in Mr. TOWNLEY's experiments with which the world was not at that time acquainted. Mr. TOWNLEY's rain-gage was fixed full ten yards above the ſurface of the earth[(f)]; which circumſtance, according to ſome later obſervations, makes a very material difference in the reſult of the experiment[(g)]. Were the obſervations to be repeated at Townley, and the rain-gage placed upon the ground, there can be no doubt but that the depth of rain would conſiderably exceed 41.516 inches; for I find from a great number of experiments, made during the laſt three

(*e*) Phil. Tranſ. abridged by Lowthorp, vol. II. p. 46. (*f*) Ibid.
(*g*) Phil. Tranſ. vol. LIX. art. 47.

years

years with two veſſels of equal dimenſions, one placed on the ground, and the other eighteen yards higher on the battlement of the hoſpital; that the quantity received in the lower veſſel exceeds that in the higher more than one-third and leſs than one-half.

6. An ingenious friend, on peruſing theſe obſervations, aſked, "Whether the fact of evaporation going on "equally well in an exhauſted receiver, was not an un-"ſurmountable objection to that theory concerning eva-"poration, which ſuppoſes a chemical ſolution of water "in air?" With a view to aſcertain this fact I made the following experiment.

Two china ſaucers, each containing three ounces of water, were accurately weighed. One of them was placed in the open air; the quickſilver in the thermometer ſtood during the experiment between 48° and 50°, the day tolerably clear with a moderate breeze. The other was put under the receiver of an air-pump; the air was exhauſted, and the piſtons occaſionally worked, to draw off any of the water which might be ſuppoſed to be converted into vapour. After four hours the ſaucers were again accurately weighed; that in the open air had loſt one drachm and eight grains; the weight of the other was not ſenſibly diminiſhed.

From

From this experiment it appears, that air is a chemical ſolvent of water, and as ſuch is undoubtedly to be conſidered as one cauſe of the evaporation of water. Heat is another cauſe of evaporation, and when raiſed to a ſufficient degree may produce this effect without the intervention of air, and the evaporation conſequently go on copiouſly in an exhauſted receiver, agreeably to the experiments of the ingenious Dr. IRVING[(i)].

The following obſervations are added as a farther illuſtration of this ſubject. Water may exiſt in air in three different ſtates. 1. In a ſtate of perfect ſolution. 2. In a ſtate of beginning precipitation. Or, 3. Completely precipitated, and falling in drops of rain.

In the firſt inſtance, where the water is in a ſtate of perfect ſolution, the air is clear, dry, heavy, and its powers of ſolution ſtill active, though it already contains a conſiderable proportion of water. In the ſecond, the air becomes moiſt, foggy, its powers of ſolution are diminiſhed, and it becomes lighter in proportion as its water is depoſited. It is a ſingular and well-atteſted fact, that it never rains in the kingdom of Peru; but that during part of the year the atmoſphere is conſtantly obſcured with

(i) Phipps's Voyage to the North Pole, p. 211.

vapours, and the whole country involved in what they call *Garuas*, or thick fogs [(i)].

It is not neceffary to point out the caufes which thus difpofe the air to depofite its diffolved water; nor to confider with what bodies air hath a ftronger affinity than with water; neither to inquire how far the electrical fluid is engaged in the procefs. It is fufficient to obferve, that fo long as thefe caufes have a general action on the air, they diminifh its power of folution, and give a damp and foggy ftate of the atmofphere; that when they operate for a confiderable proportion of the year, they produce a moift climate; and that when they more generally do not, and the air retains its moifture in a ftate of perfect folution, the climate is dry. Confequently, that the moifture or drynefs of a climate, do not fo much depend upon the abfolute quantity of water which is contained in the air, as upon the air being in a ftate of perfect or imperfect folution. During long continued fummer droughts, a very large proportion of water is diffolved in the air; notwithftanding this, the air is ftill dry, and continues to be fo as long as the water remains in a ftate of perfect folution; but no fooner are the powers of folution diminifhed, than what was before a dry, now becomes a moift climate.

(*i*) D'Ulloa's Voyage to South America, vol. II. p. 69.

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In the third inſtance, the diſſolved water may be either ſlowly precipitated and fall in drizzling rain, or it may be more powerfully diſcharged in briſk rain; or there may be partial and ſudden precipitations from particular regions, while other parts of the atmoſphere ſtill retain their water in a ſtate of perfect ſolution. Heavy thunder-ſhowers are the moſt remarkable inſtances of partial, ſudden, and copious precipitations.

Liverpool, Nov. 20, 1776.

XIV. *An Account of Persons who could not distinguish Colours. By Mr.* Joseph Huddart, *in a Letter to the Rev.* Joseph Priestley, *LL.D. F. R. S.*

London,
Jan. 15, 1777.

SIR,

Read Feb. 13, 1777.

WHEN I had the pleasure of waiting on you last winter, I had hopes before now of giving you a more perfect account of the peculiarity of vision which I then mentioned to you, in a person of my acquaintance in the North: however, if I give you now the best I am able, I persuade myself you will pardon the delay.

I promised to procure you a written account from the person himself, but this I was unfortunately disappointed in, by his dying suddenly of a pleurisy a short time after my return to the country.

You will recollect I told you that this person lived at Mary-port in Cumberland, near which place, *viz.* at Allonby, I myself live, and having known him about ten years have had frequent opportunities of conversing with him.

His name was HARRIS, by trade a ſhoe-maker. I had often heard from others that he could diſcern the form and magnitude of all objects very diſtinctly, but could not diſtinguiſh colours. This report having excited my curioſity, I converſed with him frequently on the ſubject. The account he gave was this: That he had reaſon to believe other perſons ſaw ſomething in objects which he could not ſee; that their language ſeemed to mark qualities with confidence and preciſion, which he could only gueſs at with heſitation, and frequently with error. His firſt ſuſpicion of this aroſe when he was about four years old. Having by accident found in the ſtreet a child's ſtocking, he carried it to a neighbouring houſe to inquire for the owner: he obſerved the people called it a *red* ſtocking, though he did not underſtand why they gave it that denomination, as he himſelf thought it completely deſcribed by being called *a ſtocking*. The circumſtance, however, remained in his memory, and together with ſubſequent obſervations led him to the knowledge of his defect. As the idea of colours is among the firſt that enters the mind, it may perhaps ſeem extraordinary that he did not obſerve his want of it ſtill earlier. This, however, may in ſome meaſure be accounted for from the circumſtance of his family being quakers, among

whom

whom a general uniformity of colours is known to prevail.

He obferved alfo that, when young, other children could difcern cherries on a tree by fome pretended difference of colour, though he could only diftinguifh them from the leaves by their difference of fize and fhape. He obferved alfo, that by means of this difference of colour they could fee the cherries at a greater diftance than he could, though he could fee other objects at as great a diftance as they; that is, where the fight was not affifted by the colour. Large objects he could fee as well as other perfons; and even the fmaller ones if they were not enveloped in other things, as in the cafe of cherries among the leaves.

I believe he could never do more than guefs the name of any colour; yet he could diftinguifh white from black, or black from any light or bright colour. Dove or ftraw-colour he called white, and different colours he frequently called by the fame name: yet he could difcern a difference between them when placed together. In general, colours of an equal degree of brightnefs, however they might otherwife differ, he frequently confounded together. Yet a ftriped ribbon he could diftinguifh from a plain one; but he could not tell what the colours were with any tolerable exactnefs. Dark colours

in general he often miſtook for black, but never imagined white to be a dark colour, nor a dark to be a white colour.

He was an intelligent man, and very deſirous of underſtanding the nature of light and colours, for which end he had attended a courſe of lectures in natural philoſophy.

He had two brothers in the ſame circumſtances as to ſight; and two other brothers and ſiſters who, as well as their parents, had nothing of this defect.

One of the firſt mentioned brothers, who is now living, is maſter of a trading veſſel belonging to Maryport. I met with him in December 1776, at Dublin, and took the opportunity of converſing with him. I wiſhed to try his capacity to diſtinguiſh the colours in a priſm, but not having one by me, I aſked him, Whether he had ever ſeen a rain-bow? He replied, He had often, and could diſtinguiſh the different colours; meaning only, that it was compoſed of different colours, for he could not tell what they were.

I then procured and ſhewed him a piece of ribbon: he immediately, without any difficulty, pronounced it a ſtriped and not a plain ribbon. He then attempted to name the different ſtripes: the ſeveral ſtripes of white he uniformly, and without heſitation, called white: the four black ſtripes he was deceived in, for three of them

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he thought brown, though they were exactly of the fame fhade with the other, which he properly called black. He fpoke, however, with diffidence as to all thofe ftripes; and it muft be owned, the black was not very diftinct: the light green he called yellow; but he was not very pofitive: he faid, "I think this is what you call yellow." The middle ftripe, which had a flight tinge of red, he called a fort of blue. But he was moft of all deceived by the orange colour; of this he fpoke very confidently, faying, "This is the colour of grafs; this is green." I alfo fhewed him a great variety of ribbons, the colour of which he fometimes named rightly, and fometimes as differently as poffible from the true colours.

I afked him, Whether he imagined it poffible for all the various colours he faw, to be mere difference of light and fhade; whether he thought they could be various degrees between white and black; and that all colours could be compofed of thefe two mixtures only? With fome hefitation he replied, No, he did imagine there was fome other difference.

I could not conveniently procure from this perfon an account in writing; but I have given his own words, having fet them down in writing immediately. Befides, as this converfation happened only the 10th of laft month

months, it is ſtill freſh in my memory. I have endeavoured to give a faithful account of this matter, and not to render it more wonderful than it really is.

It is proper to add, that the experiment of the ſtriped ribbon was made in the day-time, and in a good light.

I am, SIR, &c.

XV. *A new Theory of the Rotatory Motion of Bodies affected by Forces disturbing such Motion. By Mr.* John Landen, *F. R. S.*

Read Feb. 20, 1777.

I AM induced to consider this paper as not unworthy the notice of this Society, through a persuasion that the theory herein contained will conduce to the improvement of science, by enabling the reader to form a true idea, and accordingly to make a computation of the motion (or change) of the axis about which a body having a rotatory motion will turn, or have a tendency to turn, upon being affected by a force disturbing its rotation; particularly of the motion of the earth's axis arising from the attraction of the Sun and Moon on the protuberant matter of the earth above its greatest inscribed sphere: which compound motion, I conceive, has not been rightly explained by any one of the eminent mathematicians whose writings on the same subject have come to my hands. Whether in this essay I have really succeeded better than other writers who have attempted an explanation of such motion, I submit to gentlemen well versed in mechanics to determine.

1. Fig.

1. Fig. 1. Let the fphere ADBE, whofe radius is r, revolve uniformly about the diameter ACB as an axis, with the angular velocity c, meafured at D or E, the motion being according to the order of the letters DGEH in the fection at right angles to ACB, fig. 2.; and, whilft it is fo revolving, let the pole A be impelled by fome inftantaneous percuffive force to turn about the diameter DCE, from A towards H, with the velocity w. It is propofed to find the new axis about which the fphere will revolve after receiving fuch impulfe.

Calling al, parallel to DC, x; cl will be $=\sqrt{r^2-x^2}$: the velocity of the point a (about ACB) before the impulfe on A will be $=\frac{cx}{r}$; and the velocity (about DCE) given to the fame point (a) by the faid impulfe will be $=\frac{w\sqrt{r^2-x^2}}{r}$. Which velocities of the point a being in contrary directions, if it be fo fituated that they be equal, then, one deftroying the other, that point will ftop and become one of the new poles fought, about which the former poles A and B will revolve with the velocity w; and the points D and E will revolve with the fame velocity (c) as before the perturbating impulfe on the point A; but inftead of defcribing the great circle DGEH, their motion will be about the new axis ab; about which they (as well as the points A and B) will defcribe leffer circles parallel

to the great circle de, in which the points d and e (de being at right angles to ab) will revolve about the ſame axis (ab) with the velocity $\sqrt{c^2+w^2}$. Which being denoted by e, and m and n being put for the ſine and coſine of the angle ACa to the radius 1, me will be $=w$, $ne=c$, and conſequently $mne^2=cw$.

Now taking $\frac{cx}{r}=\frac{w\sqrt{r^2-x^2}}{r}$, in order to find that new axis ab, we have from that equation $x=\frac{rw}{\sqrt{c^2+w^2}}=\frac{rw}{e}=al$.

Moreover it is obvious, that if a ſpheroid, a cylinder, or any other body, whoſe center of gravity is C and proper axis ACB, were, whilſt revolving about that axis with the ſame angular velocity (c), to receive ſuch an impulſe as inſtantly to give the point A the angular velocity w about DCE; the axis about which that ſpheroid, cylinder, or other body, immediately after the impulſe, would revolve, or would have a tendency to revolve, would be the ſame line ab.

The great circle de (fig. 1.) and any other great circle ſo ſituated with reſpect to the axis of any revolving ſphere, I ſhall denominate the *mid-circle*.

2. In the manner above deſcribed the poles of the ſphere are by the inſtantaneous impulſe on the point A inſtantly changed from A and B to a and b. But if, inſtead of ſuch impulſe, a continued attractive force F (like that of gravity)

vity) acted at A fig. 3. and at the new poles $\acute{a}$, $\acute{\acute{a}}$, &c. as they become ſuch by a ſucceſſive change cauſed by ſuch continued action of the force F urging the ſphere at every inſtant to revolve about the diameter $\acute{d}\acute{e}$, or $d''e''$, &c. of the contemporary mid-circle, the new pole (a', a'', &c.) would not inſtantly be at a finite diſtance from the primitive pole A, but ſome finite time would be requiſite, that by ſuch ſucceſſive change, the pole might be varied to a finite diſtance from A: and the force F continuing invariable, the velocity (v) wherewith the pole changed its place would be expreſſed by $\frac{z}{t}$, t being the time elapſed whilſt the pole is varying from A to a, and z the length of the arc Aa. Therefore the velocity wherewith the pole will change its place during ſuch action of the force F will be expreſſed in the ſame manner as the velocity (v) of a body moving uniformly from A to a in the time t may be expreſſed; that is, in both caſes v will be $= \frac{z}{t}$. But there is a material difference between the motion of a body ſo moving from A to a and the change of place of the pole a', a'', &c. the former is permanent, and will continue to carry the body forward without the action of any force whatever; whereas the latter will inſtantly ceaſe, and the axis will keep its poſition, if the force

force F ceases to act thereon; like as the varying direction of a projectile near the earth's surface would immediately cease to change, if the force of gravity ceased to act.

It is observable, that whilst the force F acts, and the revolving sphere, in consequence of such action, every moment takes a new axis, the angular motion about the axis will continue invariable; the action of such force only altering the axis without altering the angular velocity of the sphere about it: like as the direction of a moving body is altered, without altering the velocity thereof, by an attractive force continually acting on it in a direction at right angles to that in which the body is moving. And if ever the force F shall cease to act, the sphere will instantly revolve with its primitive velocity (c) about the axis it then may have been brought to take by the preaction of that force.

The new axis, about which the sphere has such tendency to revolve at any instant during the action of the force F, I shall call the *momentary axis*; and the poles thereof the *momentary poles*.

3. From the equation $\frac{cx}{r} = \frac{w\sqrt{r^2-x^2}}{r}$ (art. 1.) we have $\frac{w}{x} = \frac{c}{\sqrt{r^2-x^2}}$. Now if a continued attractive force (F) act during the time t as above-mentioned, instead of the

inftantaneous percuffive force at A, we, according to the doctrine of fluxions, muft, inftead of w, take $\dot{w}$, or its equal F $\dot{t}$, and $\dot{x}$ inftead of x, in the expreffion $\frac{w}{x}$; therefore, in this cafe, we have $\frac{\dot{w}}{\dot{x}} = \frac{F\dot{t}}{\dot{x}} = \frac{c}{\sqrt{r^2-x^2}}$. Whence, putting z for the arc (A$\acute{a}$, or A$\acute{\acute{a}}$, &c.) whofe fine is x, and writing $\dot{z}$ for its equal $\frac{r\dot{x}}{\sqrt{r^2-x^2}}$, we get $\frac{rF\dot{t}}{\dot{z}} = c$, or $\dot{z} = \frac{rF\dot{t}}{c}$.

Hence v denoting the velocity wherewith the momentary pole ($\acute{a}$, $\acute{\acute{a}}$, &c.) changes its place during the action of the accelerative force F, we have $\dot{z} = v.\dot{t} = \frac{rF\dot{t}}{c}$, and confequently $v = \frac{rF}{c}$.

4. The value of v may alfo be determined in the following manner (fig. 4.). Conceive a very thin ftring (without weight) to have one of its ends faftened to a fixed point l and the other to a heavy particle of matter m; alfo conceive fuch particle fo to revolve with the velocity e, about the line ln, that a certain accelerative force F (like that of gravity referred to a certain direction) continually acting on the faid particle m, in a direction at right angles both to the ftring lm and to the tangent to the curve in which m is moving, the ftring fhall defcribe a conical furface. Then lm being denoted by

by r, and mo, perpendicular to ln, by q; $\frac{e^2}{q}$, the centrifugal force urging m in the direction om, will be to F as r to $\sqrt{r^2-q^2}=lo$. Therefore F muft be $=\frac{e^2\sqrt{r^2-q^2}}{rq}$. Now if, whilft m is fo revolving, the force F ceafes acting, the faid particle (m) will, it is obvious, immediately proceed to defcribe a great circle of the fphere whofe radius is r and center l, of which great circle one of the poles will be fituated in a leffer circle parallel to, and 90° diftant from, that defcribed by m during fuch action of the faid force; which pole, during fuch action, will change its place in the faid leffer circle in which it will at any time be found with a velocity (v) which will be to e as (s) the radius of the laft mentioned circle to q. But s will be $=\sqrt{r^2-q^2}$; therefore we have $v:e::\sqrt{r^2-q^2}:q$, and $\frac{\sqrt{r^2-q^2}}{q}=\frac{v}{e}$. Confequently $F=\frac{e^2\sqrt{r^2-q^2}}{rq}$ will be $=\frac{e^2}{r}\times\frac{v}{e}=\frac{ev}{r}$, and $v=\frac{rF}{e}$.

Let now m be a point on the furface of a fphere whofe center is l, and radius $lm=r$; and let the fphere revolve about an axis fo that m fhall defcribe a great circle with the velocity e. If then fuch a motive force begins to act on the fphere, that, continuing its action, the point m fhall always be urged by the invariable accelerative force F to move in a direction at right angles to the ray lm and

to the tangent to the curve which m will defcribe; that point it is obvious will, in confequence of the action of that force, defcribe a leffer circle of the fame radius (q) as that defcribed by the particle m when faftened to a ftring and acted on by the force F as above-mentioned; and the center of the fphere being always confidered as at reft, one of the momentary poles of the fphere will defcribe a circle whofe radius will be $= \sqrt{r^2 - q^2}$ parallel to, and 90° diftant from, that defcribed by the point m. For if the faid force were to ceafe acting, that point of the fphere would defcribe a great circle, as would the particle m at the ftring in the like cafe; and therefore both the faid particle and the point m of the fphere at every inftant having the fame tendency, and being acted on by equal accelerative forces, the effect will be the fame with refpect to the motion of each. Confequently, v being put to denote the velocity wherewith the momentary pole changes its place in the circle which it will defcribe whilft the motive force producing the accelerative force F acts on m as juft now mentioned, v will be $= \frac{rF}{e}$, the fame as in the preceding article, e here denoting that velocity which we there denoted by c.

5. Referring the point of action of the perturbating force to the mid-circle we have not hitherto confidered

that point as varied with a greater or leſs velocity than (e) that of the point m; that is, with reference to ſuch circle we have always conſidered the point m as the point of action. But it is obvious, that, *cæteris paribus*, the point of action with reſpect to the mid-circle (which point we will now denote by q) may be varied with a velocity greater or leſs than e; and that, *cæteris paribus*, the velocity (v) of the momentary pole will be the ſame with what velocity ſoever (q) the point of action of the force F be varied; the direction in which that force acts being always at right angles to the ray (lq) from the center of the ſphere, and to the tangent to the curve deſcribed by (q) ſuch point of action.

Yet, although v continues the ſame whether, *cæteris paribus*, (u) the velocity of the point q be greater, equal to, or leſs than e, the immoveable circle in which the momentary pole will be found will not continue the ſame; that circle being greater, equal to, or leſs than the circle whoſe radius is $\sqrt{r^2-q^2}$ according as u is leſs, equal to, or greater than e, as will be made more evident by what follows.

6. Fig. 5. Let $\acute{p}$ (in the great circle R $\acute{p}$ Q $\acute{q}$ T) be one of the poles of the axis about which the ſphere RSTV, whoſe radius is r, is revolving (according to the order of the letters V $\acute{q}$ S) with the angular velocity e, meaſured at

4 the

the diſtance r from the axis; and whilſt it is ſo revolving let the ſaid pole be urged to turn about a diameter of the mid-circle Vq'S towards q', by an accelerative force F, and let ſuch force continue to act on the ſucceſſive new poles p'', p''', &c. as they become ſuch, always urging the ſphere to turn about a diameter of the contemporary mid-circle, whilſt the direction in which ſuch perturbating force acts is regulated in the following manner.

Conceive the ſaid revolving ſphere to be ſurrounded by an immoveable concave ſphere of the ſame radius r. Then the momentary pole (p', p'', p''', &c.) will always be found in ſome curve $P'P''P'''$ &c. in the ſaid concave ſphere, and in ſome curve $p'p''p'''$ &c. on the revolving ſphere; which laſt mentioned curve will continually touch and roll along the other curve $P'P''P'''$ &c. on the immoveable ſphere, the force F and the direction in which it acts varying in any manner whatever. Let F be invariable; then, it is obvious, the two curves ſo touching each other will be circles; and if great circles $P'q'$, $P''q''$, $P'''q'''$, &c. be deſcribed on the ſurface of the immoveable ſphere whoſe planes ſhall be at right angles to the plane of the circle $P'P''P'''$ &c. the points q', q'', q''', &c.

 therein,

therein, each 90° diſtant from P', P'', P''', &c. reſpectively, will be in a circle ($q'q''q'''$ &c.) parallel to the ſaid circle $P'P''P'''$ &c. Now as a regulation to the direction in which the force F ſhall urge the momentary pole, let that direction be always a tangent to the great circle ſo paſſing through that pole and the correſpondent point q', q'', or q''', &c. whilſt the arcs $q'q''$, $q'q'''$, &c. are to the arcs $P'P''$, $P'P'''$, &c. reſpectively in the conſtant ratio of u to v.

The direction in which the force F acts being ſo regulated, it is obvious that the radius of the circle $P'P''P'''$ &c. being denoted by b, the radius of the circle $q'q''q'''$ &c. will be $=\sqrt{r^2-b^2}$, the diſtance of theſe parallel circles being 90°. Therefore their peripheries being as the velocities (v and u) with which they are deſcribed, their *radii* (b and $\sqrt{r^2-b^2}$) will be in the ratio of the ſaid velocities; that is $v:u::b:\sqrt{r^2-b^2}$; whence, $\frac{u}{v}$ being $=\frac{\sqrt{r^2-b^2}}{b}$, b, the radius of the circle $P'P''P'''$ &c. is found $=\frac{rv}{\sqrt{u^2+v^2}}$ $=\frac{r}{\sqrt{1+\frac{e^2u^2}{r^2F^2}}}$; and $\sqrt{r^2-b^2}$ the radius of the circle $q'q''q'''$ &c. $=\frac{ru}{\sqrt{u^2+v^2}}=\frac{r}{\sqrt{1+\frac{r^2F^2}{e^2u^2}}}$, v being $=\frac{rF}{e}$, the velocity where-

with

with the momentary pole p'', p''', &c. changes its place.

Confequently, if p'R be an arc in the faid immoveable concave fphere whofe fine is $\frac{rv}{\sqrt{u^2+v^2}}=\frac{r}{\sqrt{1+\frac{e^2u^2}{r^2F^2}}}$, the great circles $q'P'$, $q''P''$, $q'''P'''$, &c. will interfect each other at the point R.

7. Moreover, the force F being invariable and acting as expreffed in the preceding article, the primitive pole p' and the momentary poles p'', p''', &c. will all be found in a circle $p'p''p'''$ &c. defcribed upon the furface of the revolving fphere, as obferved in that article; which circle, during the action of the force of F, will (as is alfo obferved in the faid article) always touch and roll along the immoveable circle (P′P″P‴ &c.) whofe radius we have juft now found $=\frac{rv}{\sqrt{u^2+v^2}}=\frac{r}{\sqrt{1+\frac{e^2u^2}{r^2F^2}}}$; the point of contact being always the momentary pole.

Let the fine of the arc $p'Q'$ of the great circle R$p'Q'q'$T in the revolving fphere be equal to k, the radius of the faid circle $p'p''p'''$ &c. then will the point Q and its oppofite point (O) in the furface of the faid fphere, during the action of the force F, defcribe circles in the fur-

3 rounding

rounding immoveable concave ſphere parallel to ($P\overset{\prime}{P}\overset{\prime\prime}{P}\overset{\prime\prime\prime}{}$ &c.) the circle deſcribed by the momentary pole $\overset{\prime\prime}{p}$, $\overset{\prime\prime\prime}{p}$, &c, in the ſame concave ſphere. And ſuch point Q and its oppoſite point (o) being continually urged by the force F in directions at right angles to the tangents to the arcs they deſcribe, their velocity will continue the ſame as before the action of the ſaid force commenced; which velocity, and the radius of the ſaid circle $\overset{\prime}{p}\overset{\prime\prime}{p}\overset{\prime\prime\prime}{p}$ &c. will be determined by the following computation.

That radius being denoted by k, we have $r : k :: e : \frac{ek}{r}$, the velocity of the point Q before the action of the force F commenced; and $b : v :: \text{K} : \frac{\text{K}v}{b}$, the velocity of the ſame point (Q) during the action of that force, K being put for the ſine of the arc QR; therefore the velocity of Q continuing the ſame during the action of F as before, we have $\frac{ek}{r} = \frac{\text{K}v}{b}$. But K is the ſine of the ſum of the arcs $\text{R}\overset{\prime}{\text{P}}$, $\overset{\prime}{\text{P}}\text{Q}$, whoſe ſines are b and k reſpectively; therefore $\frac{b\sqrt{r^2-k^2}}{r} + \frac{k\sqrt{r^2-b^2}}{r}$ will be $= \text{K}$; and by ſubſtitution we get $\frac{ek}{r} = \frac{v\sqrt{r^2-k^2}}{r} + \frac{kv\sqrt{r^2-b^2}}{r} = \frac{v\sqrt{r^2-k^2}}{r} + \frac{ku}{r}$, $\frac{\sqrt{r^2-b^2}}{b}$ being $= \frac{u}{v}$ by the preceding article. Hence we find $k =$

rv

$\frac{rv}{\sqrt{\overline{e-u}|^2+v^2}}$; and it follows, that $\frac{ev}{\sqrt{\overline{e-u}|^2+v^2}}$ $\left(=\frac{eF}{r}\right)$ will be equal to the velocity of the point Q, and likewise of its opposite point (O) in the surface of the sphere. It also follows, that K, the radius of each of the circles described by those points, during the action of the force F will be equal to $\frac{rev}{\sqrt{u^2+v^2}\times\sqrt{\overline{e-u}|^2+v^2}}$.

By what is done it appears, that during the action of the force F, the motion of the revolving sphere will be regulated by the circle $\overset{\prime}{p}\overset{\prime\prime}{p}\overset{\prime\prime\prime}{p}$ &c. thereon (whose radius is $\frac{rv}{\sqrt{\overline{e-u}|^2+v^2}}=\frac{r}{\sqrt{1+\frac{e^2\overline{e-u}|^2}{r^2F^2}}}$) continually touching and rolling along the immoveable circle $\overset{\prime}{P}\overset{\prime\prime}{P}\overset{\prime\prime\prime}{P}$ &c. (whose radius is $\frac{rv}{\sqrt{u^2+v^2}}=\frac{r}{\sqrt{1+\frac{e^2u^2}{r^2F^2}}}$) so that the velocity of the point of contact be $=v=\frac{rF}{e}$. Considering the point Q as always urged from the points $\overset{\prime}{P}$, $\overset{\prime\prime}{P}$, $\overset{\prime\prime\prime}{P}$, &c. and consequently its opposite point (O) towards those points, it is necessary to observe, that according as u is less or greater than e, the arc $\overset{\prime}{P}Q$ (whose sine is $\frac{rv}{\sqrt{\overline{e-u}|^2+v^2}}$) will be less or greater than 90°; and the point (O) opposite to Q on the surface of the sphere will accordingly be at a greater or less distance than 90° from $\overset{\prime}{P}$.

If

If u be negative the arc PR whofe fine is $\frac{rv}{\sqrt{u^2+v^2}}$ will be greater than 90°.

8. The motion of the fphere according to the regulation in the preceding article is one motion compounded of the primitive motion of the fphere and the motion generated by the action of the force F. But conceiving $\left(\frac{ev}{\sqrt{\overline{e-u}|^2+v^2}}\right)$ the velocity of the point Q to arife from an impulfe given to it whilft the fphere revolved about an axis of which Q was an immoveable pole before fuch impulfe, and about which the mid-circle correfponding to that primitive axis revolved with the angular velocity $\frac{e.\overline{e-u}}{\sqrt{\overline{e-u}|^2+v^2}}$ (a); and confidering that the force F, continually acting at right angles to the momentary direction of the point Q and to the plane of the faid mid-circle, only ferves to alter the pofition of the faid primitive axis; we may, by the help of what is done above, explain the motion which the fphere will have, during the action of the force F, fo as to retain in our ideas the two primitive motions (one about the axis QO, and the other about a diameter at right angles to that axis) as remaining diftinct and unaltered.

(a) Denoting this by c and the velocity of Q by d, $\sqrt{c^2+d^2}$ is $= e$, agreeable to art. 1.

Fig.

Fig. 6. Let ED be a great circle on the revolving ſphere, of which Q is a pole, and let a ſmaller circle DL parallel to (MQ) that which we have found will be deſcribed by the point Q, be drawn on the immoveable concave ſphere ſo as to touch that great circle in the point (D) where the great circle QP′R cuts it; the radius of which leſſer circle will be $(=\sqrt{r^2-K^2}=)\frac{rv^2 \backsim ru.\overline{e-u}}{\sqrt{u^2+v^2}\times\sqrt{\overline{e-u}|^2+v^2}}$. Then the revolving ſphere, during the action of the force F, will ſo move, that the firſt mentioned great circle (ED) ſhall continually touch and roll along the ſaid leſſer circle DL, the velocity of the point of contact (along that circle) being $=\frac{v^2 \backsim u.\overline{e-u}}{\sqrt{\overline{e-u}|^2+v^2}}$ (*b*), and the ſphere at the ſame time turning about the axis of which Q is a pole with the primitive angular velocity $\frac{\overline{e.e \backsim u}}{\sqrt{\overline{e-u}|^2+v^2}}$.

Thus the primitive motion about the axis of which Q is a pole is preſerved diſtinct, whilſt that pole proceeds deſcribing a circle, whoſe radius is $\frac{rev}{\sqrt{u^2+v^2}\times\sqrt{\overline{e-u}|^2+v^2}}$, with the velocity $\frac{ev}{\sqrt{\overline{e-u}|^2+v^2}}$ which we ſuppoſed given to it.

(*b*) This is to the velocity of the point Q as $\sqrt{r^2-K^2}$ to K; that is, as the radii of the arcs deſcribed.

It is obſervable, that the laſt mentioned velocity will, according to this regulation of the motion, be to the primitive angular velocity about the axis of which Q is a pole, as v to $e-u$, or as v to $u-e$, according as u is leſs or greater than e; that is, according as the arc $P'Q$ is leſs or greater than 90°.

9. From what has been ſaid it follows, that denoting the two primitive angular velocities $\frac{e\cdot\overline{e \backsim u}}{\sqrt{\overline{e-u}^2+v^2}}$ and $\frac{ev}{\sqrt{\overline{e-u}^2+v^2}}$ (ſpecified in the preceding article) by c and d reſpectively, the radius (fig. 5.) of the circle $p'p''p'''$ &c. (or ſine of the arc $P'Q = P''Q$, &c.) will be $= \frac{dr}{e}$; the radius of the circle $P'P''P'''$ &c. (or ſine of the arc $P'R = P''R$, &c.) $= \frac{dr^2F}{e\sqrt{d^2e^2 \mp 2cdrF + r^2F^2}}$: a great circle paſſing through the primitive poles O and Q, on the revolving ſphere, will turn from the poſition ORQ with the velocity $\frac{rF}{d}$ meaſured at the mid-circle, or with the velocity $\frac{drF}{\sqrt{d^2e^2 \mp 2cdrF + r^2F^2}}$ meaſured at the fixed point R; whilſt thoſe poles deſcribe, with the velocity d, circles parallel to $P'P''P'''$ &c. the radius (K) of each of the circles (fig. 6.) ſo deſcribed

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being $= \frac{d^2 r}{\sqrt{d^2 e^2 \mp 2cdrF + r^2 F^2}}$: the radius $(\sqrt{r^2 - K^2})$ of the circle DL will be $= \frac{r \times \overline{cd \overset{\backsim}{+} rF}}{\sqrt{d^2 e^2 \mp 2edrF + r^2 F^2}}$; and the velocity $\left(\frac{v^2 \backsim e.\overline{e-u}}{\sqrt{\overline{e-u}|^2 + v^2}}\right)$ along the said circle DL $= c \overset{\backsim}{+} \frac{rF}{d}$: the upper or lower of the double signs taking place according as u $(= e \mp \frac{crF}{de})$ is less or greater than e; that is, according as the arc PQ (whose sine is $= \frac{dr}{e}$) is less or greater than 90°.

10. As an instance of the use of the preceding conclusions, I will now apply them in the solution of a very interesting problem, which I have not before seen solved.

Suppose a given spheroid, whilst revolving uniformly about its proper axis, with a given angular velocity, to be suddenly urged by some percussive force to turn, with some given angular velocity, about a diameter of its equator; it is proposed to explain the rotatory motion of the spheroid consequent to the impulse so received.

Fig. 7, 8. Let DOEQ be the spheroid, whose semi-axis CO = CQ is $= b$, and equatorial radius CD = CE $= r$; and supposing it before the impulse to revolve about its proper axis OQ with the angular velocity c, measured at the distance r from the axis, let the poles (O and Q) be suddenly urged by some percussive force to turn about a

 diameter

diameter of the equator of the ſpheroid, with the angular velocity d, likewiſe meaſured at the diſtance r from that diameter. Upon receiving ſuch impulſe, the ſpheroid will take a new axis of motion, which will be a momentary one; ſuppoſe ſuch new axis to be $pc\pi$ (c). Then the particles of the ſpheroid being urged (or having a tendency) to turn about that axis with the angular velocity $\sqrt{c^2+d^2}$, (which we will denote by e) their joint centrifugal force will ſo urge the ſpheroid to turn about that diameter of the equator which ſhall be at right angles to the momentary axis $pc\pi$, that the accelerative force of the point D of the equator to turn it about the ſaid diameter according to the order of the letters DQE will (as appears by what is proved in art. 1. and in the Appendix annexed hereto) be $= \frac{cd}{r} \times \frac{r^2-b^2}{r^2+b^2}$ or $\frac{cd}{r} \times \frac{b^2-r^2}{r^2+b^2}$ according as b is leſs or greater than r: and it follows from hence and what is proved in art. 3. and 4. that v, the angular velocity (at the diſtance r from C) with which the momentary pole p will change its place, will accordingly be $=$ $\frac{cd}{e} \times \frac{r^2-b^2}{r^2+b^2}$ or $\frac{cd}{e} \times \frac{b^2-r^2}{r^2+b^2}$.

(c) To find the poſition of this axis ſee art. 1. by which the ſine of the angle ccp (to the radius r) is found $= \frac{dr}{e}$.

More-

Moreover, referring to our obſervation in art. 8. let $u-e$ be to $\frac{cd}{e}\times\frac{r^2-b^2}{r^2+b^2}$ (the value of v) as c to d, u being greater than e; or let $e-u$ be to $\frac{cd}{e}\times\frac{b^2-r^2}{r^2+b^2}$ as c to d, u being leſs than e: whence, in both caſes, we ſhall have the ſame expreſſion $\left(\frac{c^2}{e}\times\frac{r^2-b^2}{r^2+b^2}\right)$ for the value of $u-e$; and conſequently u, in both caſes, will be $= e + \frac{c^2}{e}\times\frac{r^2-b^2}{r^2+b^2}$.

Conceive now a ſpherical ſurface without matter, having the ſame center and radius as the equator DE, to be carried about with the revolving ſpheroid; and ſuppoſe a ſphere, whoſe radius is r, to revolve about an axis pCπ with the angular velocity e, and, whilſt it is ſo revolving, let an accelerative force (F) equal to $\frac{cd}{r}\times\frac{r^2-b^2}{r^2+b^2}$ or $\frac{cd}{r}\times\frac{b^2-r^2}{r^2+b^2}$, according as b is leſs or greater than r, urge the pole p, and the ſucceſſive momentary poles as they become ſuch, to turn about a diameter of the contemporary mid-circle in the manner expreſſed in art. 6. u being to v as $e+\frac{c^2}{e}\times\frac{r^2-b^2}{r^2+b^2}$ to $\frac{cd}{e}\times\frac{r^2-b^2}{r^2+b^2}$ or as $e+\frac{c^2}{e}\times\frac{r^2-b^2}{r^2+b^2}$ to $\frac{cd}{e}\times\frac{b^2-r^2}{r^2+b^2}$, according as b is leſs, or greater than r. Then will the motion of the ſurface of this ſphere be exactly the ſame as the motion of the ſaid ſpherical ſurface carried about with the revolving

ſpheroid

fpheroid after receiving the impulfe of the percuffive force. Therefore, having reference to our conclufions in the preceding articles, we, by fubftitution, readily obtain folution to our problem.

By fubftituting properly $\frac{cd}{r} \times \frac{r^2 - b^2}{r^2 + b^2}$ or $\frac{cd}{r} \times \frac{b^2 - r^2}{r^2 + b^2}$ for F, we find,

$$\frac{d^2 r}{\sqrt{d^2 e^2 \mp 2cdr\mathrm{F} + r^2 \mathrm{F}^2}} = \frac{dr}{c} \times \frac{r^2 + b^2}{\sqrt{4r^4 + \overline{r^2 + b^2}|^2 \times \frac{d^2}{c^2}}},$$

$$\frac{r \times \overline{cd \overset{\backsim}{+} r\mathrm{F}}}{\sqrt{d^2 e^2 \mp 2cdr\mathrm{F} + r^2 \mathrm{F}^2}} = \frac{2r^3}{\sqrt{4r^4 + \overline{r^2 + b^2}|^2 \times \frac{d^2}{c^2}}},$$

$$\text{and } c \overset{\backsim}{+} \frac{r\mathrm{F}}{d} = \frac{2r^2 c}{r^2 + b^2}.$$

Which equations, refpect being had to the conclufions in art. 8. and 9. indicate that, whether b be lefs or greater than r, if an immoveable circle DL, whofe radius is $= \frac{2r^3}{\sqrt{4r^4 + \overline{r^2 + b^2}|^2 \times \frac{d^2}{c^2}}}$, be conceived to be defcribed in a plane inclined to the plane of the equator of the fpheroid (before the impulfe) in an angle whofe fine (to the radius r) is $= \frac{dr}{c} \times \frac{r^2 + b^2}{\sqrt{4r^4 + \overline{r^2 + b^2}|^2 \times \frac{d^2}{c^2}}}$, fo that the faid circle touch the faid equator in the point D in the fection OpDQE; the fpheroid after the impulfe will fo revolve, that its equator

equator will always touch and roll along the ſaid immoveable circle (DL), the velocity of the point of contact (along that circle) being $=\frac{2r^2c}{r^2+b^2}$, whilſt the ſpheroid turns about its proper axis (OQ) with the primitive angular velocity c, and the poles O and Q (by the ſaid rolling of the equator) deſcribe circles (whoſe radii are each $=\frac{bd}{c}\times\frac{r^2+b^2}{\sqrt{4r^4+\overline{r^2+b^2}|^2\times\frac{d^2}{c^2}}}$) parallel to the ſaid circle DL, with the angular velocity d (or their proper velocity $\frac{bd}{r}$) which we ſuppoſed given to them by the impulſe[(d)]. Thus the motion of the ſpheroid conſequent to the impulſe appears to be remarkably regular.

And in the very ſame manner may be explained the motion of a cylinder, whoſe primitive motion about its proper axis may be diſturbed by ſome percuſſive force in like manner as we ſuppoſed the ſpheroid diſturbed; only (inſtead of the former ſubſtitution for F) ſubſtituting for the accelerative force ariſing from the centrifugal force of the particles of the revolving cylinder its proper value $\frac{cd}{r}\times\frac{3r^2-4b^2}{3r^2+4b^2}$ (computed in our Appendix) and afterwards proceeding as we have done with regard to the ſpheroid,

(*d*) Other ways of ſolving the problem are alſo ſuggeſted by the preceding articles.

bide-

b denoting half the length of the cylinder, and r the radius of any ſection at right angles to its proper axis.

Seeing that $\left(\frac{cd}{r} \times \frac{3r^2-4b^2}{3r^2+4b^2}\right)$ the expreſſion for the ſaid accelerative force reſpecting the cylinder vaniſhes when b is $= \frac{3^{\frac{1}{2}}r}{2}$, it is manifeſt that the cylinder in that caſe will (with reſpect to its own particles) undiſturbedly revolve about any axis whatever paſſing through its center of gravity, as will a ſphere. Which remarkable property of that particular cylinder I believe has not before been taken notice of.

There are, I am aware, bodies of other forms having the like property.

The preceding articles lead us to conſider the motion of the earth's axis in a light, I preſume, more clear and ſatisfactory than any in which it has before been conſidered; but I muſt, for want of leiſure, defer making the application till ſome future opportunity; only obſerving here, that by what is done above it appears, that from the action of the Sun and Moon on the earth its axis has a diurnal motion, which I have no where ſeen explained. Which motion is not much unlike that of the axis of the revolving ſpheroid juſt now conſidered, when ($2b$) this laſt mentioned axis is many times longer than ($2r$)

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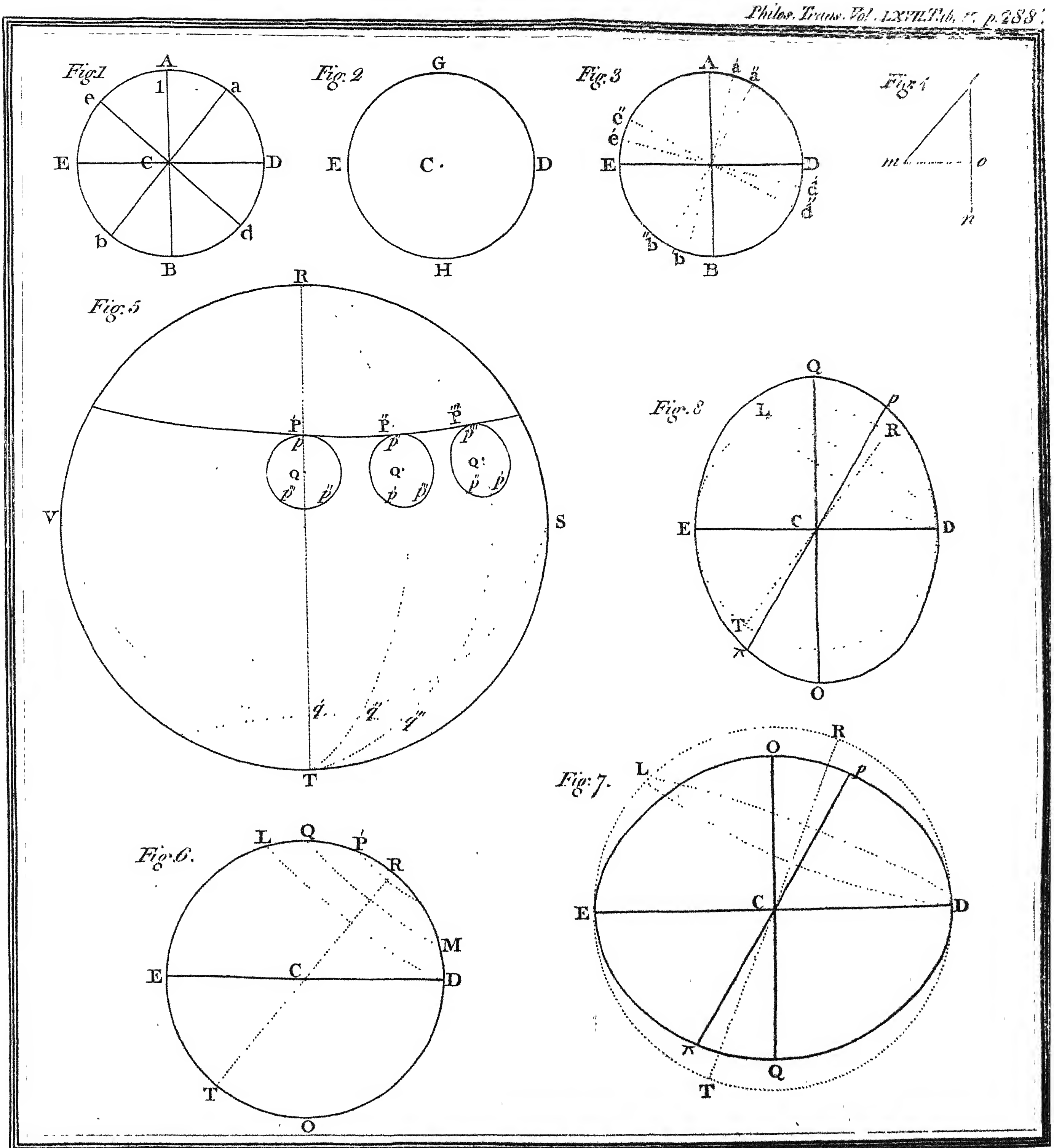
Fig. 1
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Fig. 2
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the equatorial diameter of the ſaid ſpheroid, and $\frac{d}{c}$ very ſmall.

APPENDIX.

Shewing how the joint centrifugal force of the particles of a ſpheroid or cylinder, having a rotatory motion about any momentary axis, is computed.

1. FIG. 9. Let p be a particle of matter firmly connected with the plane DOEFQG, in which the line OCQ is ſituated; and pq being a perpendicular from p to the ſaid plane, let the diſtance pq be denoted by u; alſo, the line ql being at right angles to OlCQ, let the diſtance pl be denoted by h. Then, the ſaid plane with the particle p being made to revolve about OlCQ as an axis, with the angular velocity e meaſured at the diſtance a from the ſaid axis, the velocity of p will be $=\frac{he}{a}$, and its centrifugal force from l will (by a well-known theorem) be $=\frac{he^2}{a^2}$ to make it a^2 the expreſſion being $\frac{he^2}{a^2}\times p$. Whence, by reſolving that force into two others, one in the direction qp, and the other in a direction parallel to lq, it appears that the force urging p from the plane DOEFQG will be $=\frac{ue^2}{a^2}\times p$, let the diſtance lq be what it will.

 2. The

2. The particle p being connected with the plane DOEFQG as mentioned in the preceding article, and the distance Cl being denoted by v; if p be urged directly from the said plane by a force $fu \times p$, the efficacy of that force to turn the said plane about the line HCI, therein drawn at right angles to OCQ, will (by the property of the lever) be equivalent to the force $\frac{fuv \times p}{g}$ acting on the said line OCQ at right angles to the said plane at the distance g from the point C.

Moreover it is obvious, that, *cæteris paribus*, the efficacy will be the same let the distance of q from l be what it will.

Fig. 10. Let q coincide with l; and let Ck be a line in the plane Clp continued (which plane will be at right angles to the plane DOEFQG); also, pk being at right angles to Ck, let those lines pk and Ck be denoted by w and x respectively. Then the sine and cosine of the angle kCO to the radius 1, being respectively denoted by m and n, the force $\frac{fuv \times p}{g}$ will be $= \frac{f \times p}{g} \times \overline{mn \times w^2 - x^2 + m^2 - n^2 \times wx}$.

Consequently, if each particle of any solid body, through which a line HCI and a plain DOEIFQGH may be conceived to pass, be urged from that plane by a force expressed by $fu \times p$ as above; the force which, acting on the line OCQ at the distance g from C, would be equivalent to the efficacy

efficacy of all the forces acting on the ſeveral particles of that body to turn the ſame about the line HCI will be obtained by computing the ſum of all the forces $\frac{f\times p}{g}\times\overline{mn\times\overline{w^2-x^2}+\overline{m^2-n^2}\times wx}$ acting on the ſaid body.

The computation of ſuch equivalent force will in moſt caſes be abridged by obſerving that, if pk be continued to $\overset{\prime\prime}{p}$ ſo that $k\overset{\prime\prime}{p}$ be $=kp$, the efficacy of the force on the particle $\overset{\prime\prime}{p}$, to turn the body about the line HCI in oppoſition to the force on the particle p, will be repreſented by the equivalent force $\frac{f\times\overset{\prime\prime}{p}}{g}\times\overline{mn\times\overline{x^2-w^2}+\overline{m^2-n^2}\times wx}$ acting on the line OCQ at the diſtance g from C; and that therefore the efficacy of the two forces on p and $\overset{\prime\prime}{p}$, to turn the body about HCI, will be repreſented by the equivalent force $\frac{2f\times p}{g}\times mn\times\overline{w^2-x^2}$ acting on the line OCQ, at right angles to the plane DOEIFQGH, at the diſtance g from C.

3. Fig. 11, 12. If the body be a cylinder, a ſpheroid, or the like, and its proper axis be ſituated in the line Ck, the ordinates correſponding the *abſciſſæ* kp, $k\overset{\prime\prime}{p}$, in the circular ſection hi whoſe center is k, will each be parallel to that diameter paſſing through C, about which the body will be urged to turn; and each of thoſe ordinates will be $=\sqrt{y^2-w^2}$, y being the radius of ſuch ſection.

 Therefore,

Therefore, writing $2\sqrt{y^2-w^2}$ inſtead of p, it follows that $\frac{4Af}{g}\times mn\acute{x}\times\overline{\frac{y^4}{4}-x^2y^2}$, the whole fluent of $\frac{4f\times\sqrt{y^2-w^2}}{g}\times mn\acute{x}\times\overline{w^2-x^2}\times\dot{w}$, generated $w\ (=kp=k\dot{p})$ from o becomes equal to the radius y (both x and y being conſidered as invariable) will expreſs the value of the force which, acting on the line OCQ at the diſtance g from C, would be equivalent to the force of all the particles in the ſaid ſection, whoſe thickneſs is denoted by the indefinitely ſmall quantity $\acute{x}$; the diſtance Ck being denoted by x, and A being put for (.78539) the area of a quadrant of a circle whoſe radius is 1.

4. Fig. 11. In the cylinder whoſe length is $2b$ and diameter $2r$; y being $=r$, $\overline{\frac{y^4}{4}-x^2y^2}$ will be $=r^2\times\overline{\frac{r^2}{4}-x^2}$: conſequently, the fluent of $\overline{\frac{r^2}{4}-x^2}\times\dot{x}$, generated whilſt x from o becomes $=b$, being $\frac{br^2}{4}-\frac{b^3}{3}$, we have $\frac{8Afr^2}{g}\times mn\times\overline{\frac{br^2}{4}-\frac{b^3}{3}}=\frac{fmn}{12g}\times\overline{3r^2-4b^2}\times$ M for the force which, acting as above at the diſtance g from (C) the center of gravity of the cylinder, would be equivalent to the efficacy of the forces acting as above on all the particles of the cylinder to turn it about a diameter paſſing through C, M being the maſs or content of the cylinder.

5. Fig.

5. Fig. 12. In the ſpheroid whoſe proper axis is $2b$ and equatorial diameter $2r$, y^2 being $= \frac{r^2}{b^2} \times \overline{b^2 - x^2}$, $\frac{y^4}{4} - x^2 y^2$ will be $= r^2 \times \overline{\frac{r^2}{4} - \frac{r^2 x^2}{2b^2} + \frac{r^2 x^4}{4b^4} - x^2 + \frac{x^4}{b^2}}$: conſequently, the fluent of $\frac{r^2 \dot{x}}{4} - \frac{r^2 x^2 \dot{x}}{2b^2} + \frac{r^2 x^4 \dot{x}}{4b^4} - x^2 \dot{x} + \frac{x^4 \dot{x}}{b^2}$, generated whilſt x from o becomes $= b$, being $\frac{r^2 b}{4} - \frac{r^2 b}{6} + \frac{r^2 b}{20} - \frac{b^3}{3} + \frac{b^3}{5} = \frac{2}{15} \times \overline{r^2 b - b^3}$, we have $\frac{16 \text{A} f r^2}{15 g} \times mn \times \overline{r^2 b - b^3} = \frac{fmn}{5g} \times \overline{r^2 - b^2} \times \text{s}$ for the force which, acting at the diſtance g from C the center of the ſpheroid, would be equivalent to the efficacy of the forces acting as above on all the particles of the ſpheroid to turn it about a diameter of its equator, S being the maſs or content of the ſpheroid.

Theſe equivalent forces are diſtinguiſhed by the name of motive forces; the correſpondent accelerative forces are computed in the following articles.

6. Fig. 13. The body being a ſpheroid whoſe center is C, and whoſe proper axis PN is $= 2b$ and equatorial diameter AB $= 2r$; let F be the accelerative force of a particle at the diſtance g from the axis about which the body is urged to turn, which axis is ſuppoſed to be a diameter of its equator. Denote Ck by x; ki by y; and let the *abſciſſa ko* and its correſpondent ordinate (parallel to the laſt mentioned axis) in the circle whoſe radius is ki be denoted

denoted by s and t respectively. Then, considering the body as urged to turn about that diameter of its equator which is at right angles to AB, the accelerative force of every particle in the said ordinate will be $= \frac{\sqrt{s^2+x^2}}{g} \times F$, and the motive force of all the particles in the same ordinate will be $= \frac{\sqrt{s^2+x^2}}{g} \times F \cdot \dot{t} s = \frac{\sqrt{s^2+x^2}}{g} \times F \dot{s} \sqrt{y^2-s^2}$; to which (by the property of the lever) a motive force $= \frac{s^2+x^2}{g^2} \times F \dot{s} \sqrt{y^2-s^2}$ acting at the distance g from the center at right angles to a ray therefrom would be equivalent. Therefore, considering x and y as invariable, and s only as variable, $\frac{4F\dot{x}}{g^2} \times$ the whole fluent of $\dot{s}\sqrt{y^2-s^2} \times \overline{s^2+x^2}$ will denote a force which, acting at the distance g from C, would be equivalent to the motive force of all the particles in the section bi whose radius is ki and thickness $\dot{x}$. Which fluent is $= Ay^2 \times \overline{x^2+\frac{y^2}{4}} = \frac{Ar^2}{b^2} \times \overline{b^2-x^2} \times \overline{x^2+\frac{r^2}{4b^2} \times \overline{b^2-x^2}}$. Consequently $\frac{8Ar^2F}{b^2g^2} \times$ the whole fluent of $\dot{x} \times \overline{b^2-x^2} \times \overline{x^2+\frac{r^2}{4b^2} \times \overline{b^2-x^2}}$ will denote a motive force which, acting at the distance g from C at right angles to a ray therefrom, would be equivalent to the whole motive force urging the spheroid to turn as above mentioned. Such equivalent force

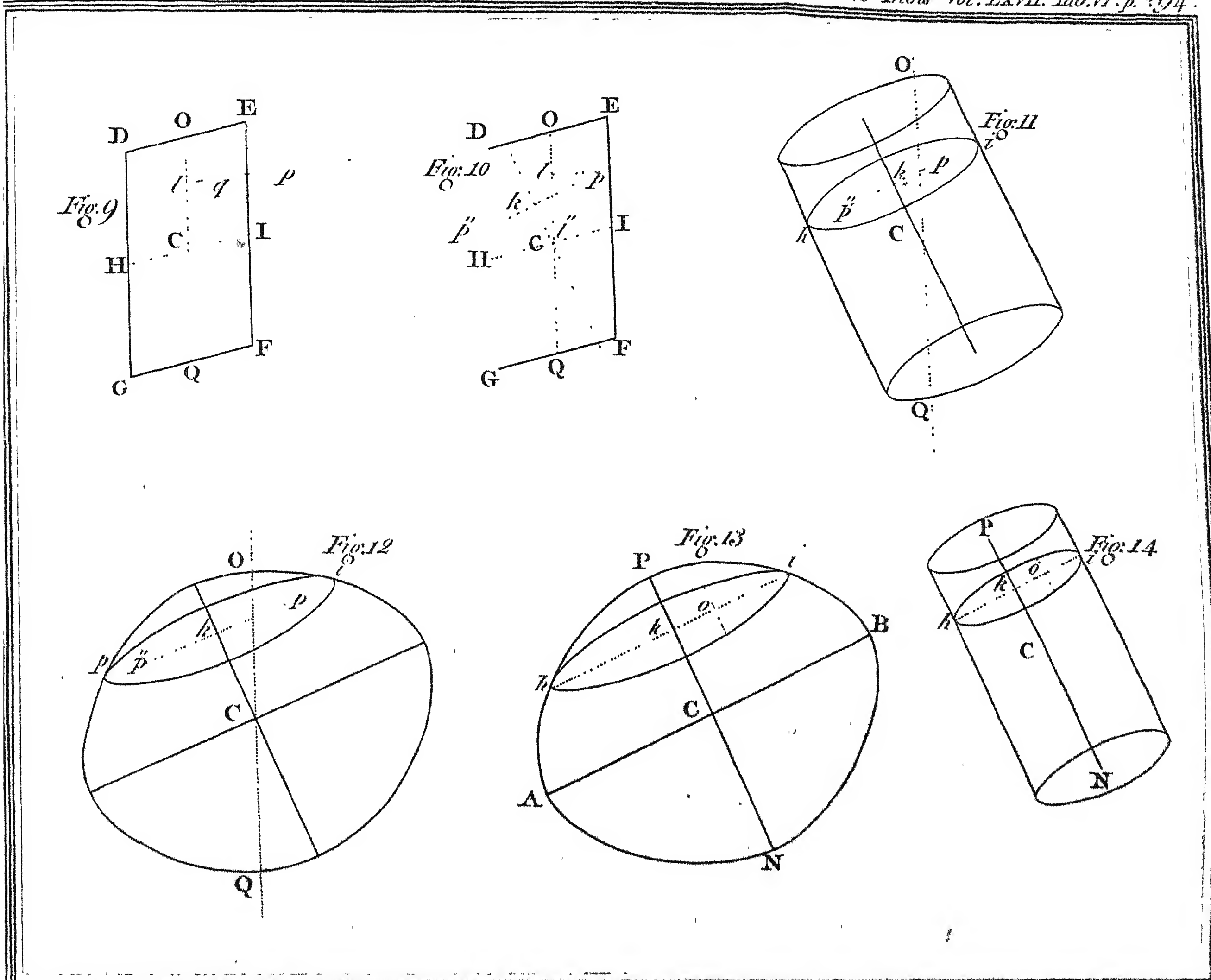
Fig:9
Fig:10
Fig:11
Fig:12
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Fig:14

force will therefore be $= \frac{16 A r^2 F}{15 g^2} \times \overline{r^4 b + r^2 b^3} = \frac{F}{5 g^2} \times \overline{r^2 + b^2} \times S$; and this being put $= \frac{fmn}{5g} \times \overline{r^2 - b^2} \times S$ (the value of the same force found in art. 5.) we find $F = fgmn \times \frac{r^2 - b^2}{r^2 + b^2}$; which will be $= \frac{gmne^2}{a^2} \times \frac{r^2 - b^2}{r^2 + b^2}$, if f be $= \frac{e^2}{a^2}$, its value computed in art. 1.

Or F will be denoted by $\frac{cd}{r} \times \frac{r^2 - b^2}{r^2 + b^2}$; if 1 be to e as m to d, and as n to c; and a and g be each $= r$.

7. Fig. 14. The body being a cylinder whose center of gravity is in C, and whose proper axis PN is $2b$ and diameter $2r$; the accelerative force (F) at the distance g from C, will in like manner be found $= \frac{gmne^2}{a^2} \times \frac{3r^2 - 4b^2}{3r^2 + 4b^2}$; the cylinder being considered as urged to turn about a diameter passing through C.

If $1 : e :: m : d :: n : c$, and a and g be each $= r$, F will be $= \frac{cd}{r} \times \frac{3r^2 - 4b^2}{3r^2 + 4b^2}$.

XVI. *Directions for making the best Composition for the Metals of reflecting Telescopes; together with a Description of the Process for grinding, polishing, and giving the great Speculum the true parabolic Curve. By Mr.* John Mudge; *communicated by* Alexander Aubert, *Esq. F. R. S.*

Read Feb. 27. March 6. and 13. 1777.

AS the method of casting, grinding, and polishing the specula of reflecting telescopes, by Mess. MOLYNEUX and HADLEY, which is published in Dr. SMITH's Optics, is what the workmen have generally followed, and is consequently well known to them; I shall in the following account avoid a repetition of the general directions there given, and only remark upon such parts of that process which I think are essentially defective, and supply them by a method of my own, which, from long and repeated trials, I have found completely to answer the purpose. After, therefore, referring to the above account for the manner of making the gages, patterns, the method of casting, as well as a great many other particulars, I will begin with

The

The best composition for the specula of reflecting telescopes.

The perfection of the metal of which the speculum should be made consists in its hardness, whiteness, and compactness; for upon these properties the reflective powers and durability of the speculum depend. And first of the hardness and whiteness of the metal. There are various compositions recommended in SMITH'S Optics, all which have however their several defects. Three parts copper and one part and one-fourth of tin will make, he says, a very hard white metal; but it is liable to be porous. This, however, is an imperfection which I shall presently shew the method of preventing; but the permanent fault of it, and which I have myself experienced, is, that it is not hard enough. The speculum of a reflecting telescope ought to have the utmost possible hardness, compatible with its being operated upon by the tool.

It is to be observed, that ever so small a quantity of tin added to melted copper destroys its perfect malleability, and at the same time produces a metal whiter and harder than copper. As the quantity of tin is increased, suppose to a fifth or fourth part, the metal becomes whiter, still harder, and consequently more friable. If the quantity of tin be further increased to a third of the

whole compoſition, it will then have its utmoſt whiteneſs; but will be rendered at the ſame time ſo exceedingly hard and brittle, that the fineſt waſhed emery upon lead or braſs will not cut it without breaking up its ſurface; and the common blue ſtones uſed in grinding the ſpeculum, will not touch it. Mr. JACKSON (ſome time ſince dead) a mathematical-inſtrument-maker, and a moſt excellent workman, told me, that the tin was increaſed to the above proportion in his metals; but that they were ſo exceedingly hard, that it coſt him an infinite deal of pains, and a journey of two hundred miles, to find out a ſtone of ſufficient hardneſs to cut it, and whoſe texture at the ſame time was fine enough not to injure its ſurface. I have ſeen ſeveral of his finiſhed metals; they were indeed perfectly hard and white; but the kind of ſtone with which he ground them he kept a ſecret.

After many experiments with various proportions of tin and copper, by gradually increaſing the former, I at laſt found that fourteen ounces and an half of grain-tin to two pounds of good Swediſh copper, made a beautiful white and very hard metal; ſo hard indeed, that the ſtones would but barely cut it, and waſhed emery on braſs or tin but juſt grind the ſurface without breaking it up; whereas the proportion of tin being increaſed by the addition of only another half ounce, the former incon-

venience

venience immediately took place. This therefore is the *maximum* in point of hardnefs.

Thus much of the two firft confiderations, the hardnefs and whitenefs of the metal; the next, and indeed the moft effential, property is its compactnefs, or its being without pores.

This compofition (though complete in the former refpects) was, as well as Dr. SMITH's, fubject every now and then to be porous; fometimes, indeed, I fucceeded in cafting a fingle metal, or perhaps two or three, without this imperfection; at other times, and moft frequently indeed, they were attended with this defect, without my being at all able to form a probable conjecture at the caufe of my fuccefs or difappointment. The pores were fo very fmall that they were not difcoverable when the metal had received a good face and figure upon the hones, nor till the laft and higheft polifh had been given; and then it frequently appeared as if dufted over with millions of microfcopic pores, which were exceedingly prejudicial in two refpects; for firft, they became in time a lodgment for a moifture which tarnifhed the furface; and fecondly, on polifhing the fpeculum, the putty neceffarily rounded off the edges of the pores, fo as to fpoil a great part of the metal, by the lofs of as much light and

 fharpnefs

ſharpneſs in the image as there were defective points of reflection in the metal.

Beſides the trouble of a great number of experiments, in order to get rid of this miſchief, and to aſcertain the cauſe to which it was owing, there was this additional inconvenience attending it, *viz.* that the fault was not diſcovered, as was obſerved before, till a great deal of trouble had been taken in grinding and even poliſhing the metal, the whole of which was rendered uſeleſs by the mortifying diſcovery of this defect.

I was extricated at laſt from this difficulty, and in ſome meaſure by accident. Having one day made a great number of experiments, and having melted down all the good copper I had or could procure; though puzzled and fatigued, yet not caring to give it up, I recollected that I had ſome metal which was reſerved out of curioſity, and was a part of one the bells of St. Andrew's which had been re-caſt. Expecting, however, very little from this groſs and uncertain compoſition, I was nevertheleſs determined to ſee what could be made of it by enriching the compoſition with a little freſh tin. Accordingly caſting a metal with it, it turned out perfectly free from pores, and in every reſpect as fine a metal as ever I ſaw.

I could not at firſt conceive to what this ſucceſs was owing; but at laſt I hit upon the real cauſe of that defect,

which had given me ſo much embarraſſment and trouble during a courſe of near a hundred experiments, and in conſequence thereof fell upon a method which ever after prevented it.

I had hitherto always melted the copper firſt, and when it was ſufficiently fuſed, I uſed to add the proportional quantity of tin; and as ſoon as the two were mixed, and the ſcoria taken off, the metal was poured into the moulds. I began to conſider that putty was calcined tin, and ſtrongly ſuſpected, that the exceſſive heat which the copper neceſſarily undergoes before fuſion, was ſufficient to reduce part of the tin to this ſtate of calcination, which therefore might fly off from the compoſition in the form of putty, at the time the metal was poured into the flaſks.

Upon this idea, after I had furniſhed myſelf with ſome more Swediſh copper and grain-tin (both which I had always before uſed) I melted the copper, and having added the tin as uſual to it, caſt the whole into an ingot: this was, as I expected, porous. I then melted it again, and as in this mixed ſtate it did not acquire half the heat which was before neceſſary to melt the copper alone, ſo it was not ſufficient to calcine the tin; the ſpeculum was then perfectly cloſe, and free from this fault; nor did I ever after, in a ſingle inſtance, meet with the above mentioned imperfection.

All

All that is neceffary, therefore, to be done to procure a metal which fhall be white, as hard as it can be wrought, and perfectly compact, is to melt two pounds of Swedifh copper, and when fo melted, to add fourteen ounces and a half of grain-tin to it; then, having taken off the fcoria, to caft it into an ingot. This metal muft be a fecond time melted to caft the fpeculum; but as it will fufe in this compound ftate with a fmall heat, and therefore will not calcine the tin into putty, it fhould be poured off as foon as it is melted, giving it no more heat than is abfolutely neceffary. It is to be obferved, however, that the fame metal, by frequent melting, lofes fomething of its hardnefs and whitenefs: when this is the cafe, it becomes neceffary to enrich the metal by the addition of a little tin, perhaps in the proportion of half an ounce to a pound. And indeed when the metal is firft made, if inftead of adding the fourteen ounces and a half of tin to the two pounds of melted copper, about one ounce of the tin were to be referved and added to it in the fucceeding melting, before it is caft off into the moulds, the compofition would be the more beautiful, and the grain of it much finer: this I know by experience to be the cafe.

The beft method for giving the melted metal a good furface is this: the moment before it is poured off, throw into the crucible a fpoonful of charcoal-duft; imme-

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diately after which the metal muſt be ſtirred with a wooden ſpatula, and poured into the moulds.

I wiſh I may not be conſidered as tedious in the above detail; but as this buſineſs cauſed me a great deal of trouble, I was willing to give ſome account of the means by which I was freed from this difficulty ever after. Perhaps, indeed, the whole of this proceſs may be unneceſſary, as many years ſince, I communicated this compoſition, and I believe at the ſame time the method of preventing the pores, to the late Mr. PETER COLLISON, a member of the Royal Society; and likewiſe two or three years ſince, at the deſire of my brother, to Mr. MICHELL. Although it be poſſible, therefore, that this method is generally known, yet, as I have frequently of late ſeen ſpecula with this defect, and obſerved metals of ſome of Mr. SHORT's teleſcopes which are not quite ſo perfect as could be wiſhed (though they are all exquiſitely figured) I was willing by this publication wholly to remove any future embarraſſment of this ſort, and to furniſh workmen with an excellent compoſition for their metals. And would the Royal Society be pleaſed to honour the proceſs with a place in their records, I know of no other method ſo proper to give this, as well as the following information, a general notoriety.

The metal being caſt, there will be no occaſion for the complicated apparatus directed by Dr. SMITH, for grinding

grinding and poliſhing it. Four tools are all that are neceſſary, *viz.* the rough grinder to work off the rough face of the metal; a braſs convex grinder, on which the metal is to receive its ſpherical figure; a bed of hones which is to perfect that figure, and to give the metal a fine ſmooth face; and a concave tool or bruiſer, with which both the braſs grinder, and the hones are to be formed. A poliſher may be conſidered as an additional tool; but as the braſs grinder is uſed for this purpoſe, and its pitchy ſurface is expeditiouſly, and without difficulty formed by the bruiſer, the apparatus is therefore not enlarged.

Of rough grinding the ſpeculum.

The tool by which the rough ſurface of the metal is rendered ſmooth and fit for the hones, is beſt made of lead, ſtiffened with about a fifth or ſixth part of tin. This tool ſhould be at leaſt a third more in diameter than the metal which is to be ground; and for one of any ſize, not leſs than an inch thick. It may be cemented upon a block of wood, in order to raiſe it higher from the bench.

This leaden tool being caſt, it muſt be fixed in the lathe, and turned as true as it is poſſible, by the gage, to the figure of the intended ſpeculum, making a hole or pit in the

the middle, as a lodgment for the emery, of about an inch diameter for a metal of four inches: when this is done, deep grooves muſt be cut acroſs its ſurface with a graver, in the manner repreſented in fig. 1. Theſe grooves will ſerve to lodge the emery, and by their means the tool will cut a great deal faſter. There is no occaſion to fear any alteration in the convexity of this tool by working the metal upon it, for the emery will bed itſelf in the lead, and ſo far arm the ſurface of it, that it will preſerve its figure and cut the metal very faſt. Any kind of low handle, fixed on the back of the metal with ſoft cement, will be ſufficient; but it ſhould cover two-thirds of its back to prevent its bending. This way of working will cut the metal faſter, and with more truth, than the method deſcribed by Dr. SMITH; for ſhould the ſurface and rough parts be attempted to be ground off by a common grind-ſtone by hand, though you did it as near the gage as poſſible, yet the metal would be ſo much out of truth when applied to the ſucceeding tool, that no time would be ſaved by it. I uſed to employ a common labourer for this purpoſe, who ſoon acquired ſuch a dexterity at working upon this tool, that in two hours time he would give a metal of four inches diameter ſo good a face and figure as even to fit it for the hones. When all the ſand-holes and irregularities on the face of the metal are ground off, and the whole ſurface

 is

is ſmooth and regularly figured, the ſpeculum is then ready for the braſs grinder, and muſt be laid aſide for the preſent.

The manner of forming the braſs-grinding tool.

The following is the method I have always purſued. Procure a round ſtout piece of Hamburgh braſs, at moſt a ſixth part larger than the metal to be poliſhed; and let it be well hammered into a degree of convexity (by the aſſiſtance of the gage) ſuitable to the intended ſpeculum. Having done this, ſcrape and clean the concave ſide ſo thoroughly that it may be well tinned all over; then caſt upon it, after it has been preſſed a proper depth into the ſand, the former compoſition of tin and lead, in ſuch quantity, that it may (for a ſpeculum of four inches diameter) be at leaſt an inch and an half thick, and with a baſe conſiderably broader than the top, in order that it may ſtand firmly upon the bench in the manner hereafter to be deſcribed. This being done, it muſt be fixed and turned in the lathe with great care, and of ſuch a convexity as exactly to ſuit the concave gage, which we ſuppoſe already made. It will be neceſſary to be more careful in forming this than the former tool, and eſpecially that no rings be left from the turning;

nor will the ſucceeding hone tool require ſo much exactneſs, as any defects in turning, will, by a method hereafter mentioned, be eaſily remedied; but any inequality or want of truth in the braſs tool will occaſion a great deal of trouble before it can be ground out by the emery. This tool muſt have a hole (ſomewhat leſs than that in the metal to be worked upon it) in the middle, quite through to the bottom. When this tool is finiſhed off in the lathe, its diameter ſhould be one-eighth wider than the metal.

How to form the bed of hones, or the third tool.

Having choſen the kind of hones, and the beſt too, of the ſort recommended in SMITH's Optics; they ſhould be cemented in ſmall pieces (in a kind of pavement agreeably to his directions) upon a thick round piece of marble, or metal made of lead and tin like the former compoſition (which is what I have always uſed) in ſuch a manner, that the lines between the ſtones may run ſtraight from one ſide to the other; ſo that, placing the teeth of a fine ſaw in each of theſe diviſions, they may be cleared from one end to the other of the cement which riſes between the ſtones. This bed of hones ſhould be at leaſt a fourth part larger than the metal which is to be ground upon it. The ſurface of the

metal upon which the hone pavement is to be cemented may or may not, as you pleaſe, be turned of a convexity ſuitable to the gage, though I have never taken that trouble. As ſoon as the hones are cemented down, and the joints cleared by the ſaw, this tool muſt be fixed in the lathe, and turned as exactly true to the gage as poſſible; which done, it muſt be laid aſide for the preſent. The next tool to be made is the bruiſer.

The manner of forming the bruiſer, the fourth and laſt tool.

This tool ſhould be likewiſe made of thick ſtout braſs like the former, perfectly ſound, about a quarter of an inch thick, and hammered as near to the gage as poſſible. It ſhould be then ſcraped, cleaned, and tinned on the convex ſide, as the former tool was on the concave, and the ſame thickneſs of lead and tin caſt upon it. The general ſhape of this ſhould differ from the former; for as that increaſed in diameter at the bottom for the ſake of ſtanding firmly, ſo this ſhould be only as broad at bottom as at top, as it is to be uſed occaſionally in both thoſe poſitions. When this tool is fixed in the lathe, and turned off concave to the convex gage with great truth

likewiſe,

likewife, its diameter ought to be the middle fize between the hones and the polifher.

Having with the lathe roughly formed the convex brafs grinder, the bed of hones, and the concave bruifers, the convex and concave brafs tools and the metal muft be wrought alternately and reciprocally upon each other with fine emery and water, fo as to keep them as nearly to the fame figure as poffible, in order to which fome wafhed emery muft be procured. This is beft done by putting it into a phial, which muft be half filled with water and well fhaken up, fo that, as it fubfides, the coarfeft may fall to the bottom firft, and the fineft remain at the top: and whenever frefh emery is laid on the tools, the beft method (which we fhould alfo obferve with the putty in polifhing) will be, to fhake gently the bottle, and pour out a fmall quantity of the turbid mixture.

Of grinding the fpeculum, the brafs tool, and the bruifer, together.

All the tools being ready, upon a firm poft in the middle of a room, you are to begin to grind the brafs convex tool with the bruifer upon it, working the latter crofsways, with ftrokes fometimes acrofs its diameter, at others a little to the right and left, and always fo fhort that the bruifers

bruiſers may not paſs above half an inch within the ſurface of the braſs tool either way, ſhifting the bruiſer round its axis every half dozen ſtrokes or thereabout. You muſt likewiſe, every now and then, ſhift your own poſition, by walking round, and working at different ſides of the braſs tool; at times the ſtrokes ſhould be carried round and round, but not much over the tool: in ſhort, they muſt be directed in ſuch a way, and the whole grinding conducted in ſuch a manner and with ſuch equability, that every part of both tools may wear equally. This habit of grinding, as well as the future one of poliſhing, will be ſoon acquired. When you have wrought in this manner about a quarter of an hour with the bruiſer upon the tool, it will be then neceſſary to change them, and, placing the bruiſer upon its bottom, to work the convex tool upon that in the ſame manner.

When by working in this equable manner, alternately with the bruiſer and tool, and occaſionally adding freſh emery, you have nearly got out all the veſtiges of the turning tool, and brought them both nearly to a figure, it will be then time to give the ſame form to the metal. This muſt be done by now and then grinding it upon the braſs tool with the ſame kind of emery, taking care however, by working the two former tools frequently together, to keep all three exactly in the ſame curve.

The

The beſt kind of handle for the metal is made of lead, a little more than double its thickneſs, and ſomewhat leſs in diameter, of about three pounds weight, with a hole in the middle (for reaſons to be ſhewn hereafter) a little larger than that in the metal: this handle ſhould be cemented on with pitch. The upper edge of this weight muſt be rounded off, that the fingers may not be hurt; and a groove, about the bigneſs of the little finger, be turned round juſt below it, for the more conveniently holding and taking the metal off the tools.

The manner of figuring the metal upon the hones.

When the bruiſer, braſs tool, and metal, are all brought to the ſame figure, and have all a true good ſurface, the next part of the proceſs is to give a correct ſpherical figure and a fine face to the metal, upon the hones. It will be neceſſary to premiſe, however, that the hones ſhould be placed in a veſſel of water, with which they ſhould be quite covered for at leaſt an hour before they are uſed, otherwiſe they will be perpetually altering their figure when the metal comes to be ground upon them. The ſame precaution is alſo neceſſary, if you are called off from the work while you are

grinding

grinding the metal, for if they be ſuffered to grow dry, the ſame inconvenience will ariſe.

In order to give a proper figure to the hones, and exactly ſuitable to that of the braſs tool, bruiſer, and metal, when the hones are fixed down to the block, ſome common flour emery (unwaſhed) with a good deal of water muſt be put upon them, and the bruiſer being placed upon the hones and rubbed thereon with a few ſtrokes and a light hand, the inequalities of the ſtone will be quickly worn off; but as a great deal of mud will be ſuddenly generated, it muſt be waſhed off every quarter of a minute with a great deal of water. By a repetition of this, two or three times, the hones (being of a very ſoft and friable ſubſtance) will be cut down to the figure, without wearing or altering the bruiſer at all. Though this buſineſs may be quickly done, and can be continued but for a few ſtrokes at a time, I need not ſay that it is neceſſary that thoſe ſtrokes be carried in the ſame direction, and with the ſame care, which was obſerved in grinding the former tools together.

As ſoon as the hones have received the general figure of the bruiſer, and all the turning ſtrokes are worn out from them, the emery muſt be carefully waſhed off; in order to which, it will be neceſſary to clear it from the joints with a bruſh under a ſtream of water. The bruiſer

and metal muſt be likewiſe cleared in the ſame manner, and with equal care, from any lurking particles of emery.

The hones being fixed down to the block, you now begin to work the bruiſer upon them with very cautious, regular, ſhort ſtrokes, forward and backward, to the right and left, turning the axis of the bruiſer in the hand while you move round the hones, by ſhifting your poſition, and walking round the block. Indeed the whole now depends upon a knack in working, which ſhould be conducted nearly in the following manner. Having placed the bruiſer on the centre of the hones, ſlide it in an equable manner forward and backward, with a ſtroke or two directly acroſs the diameter, a little on one ſide, and ſo on the other; then ſhifting your poſition an eighth part round the block, and having turned the bruiſer in your hand about as much, give it a ſtroke or two round and round, but not far over the edges of the hones, and then repeat the croſs ſtrokes as before: thoſe round ſtrokes (which ought not to be above two or three at moſt) are given every time you ſhift your own poſition and that of the metals, previous to the croſs ones, in order to take out any ſtripes either in the hones or bruiſer, which may be ſuppoſed to be occaſioned, by the ſtraight croſs ſtrokes. During the time of working, no mud muſt be ſuffered to collect upon the hones, ſo as to

deſtroy the perfect contact between the two tools; and therefore they muſt every now and then be waſhed clean by throwing ſome water upon them. When by working in this manner all the emery ſtrokes are ground off from the bruiſer, and it has acquired a good figure and clean ſurface, you may then begin with the metal upon the hones, in the ſame cautious manner, waſhing off the mud as faſt as it collects, though that will be much leſs now than when the bruiſer was ground upon them. Every now and then, however, the bruiſer muſt be rubbed gently and lightly upon the hones, which will as it were, by ſharpening them and preventing too great ſmoothneſs, occaſion them to cut the metal much faſter.

When, after having ſome time cautiouſly wrought in the manner before deſcribed, the hone-pavement has uniformly taken out all the emery ſtrokes, and given a fine face and true figure to the metal, which will be pretty well known by the great equality there is in the feel while you are working, and by which an experienced workman will form a pretty certain judgment; having proceeded thus far, I ſay, you may then try your metal, and judge of its figure by this more certain manner.

Waſh the hone pavement quite clean; then put the metal upon the center of it, and give two or three light ſtrokes

ftrokes round and round only, not carrying, however, the edges of the metal much over the hones; this will take out the order of ftraight ftrokes: then having again wafhed the hones, and placed the fpeculum upon their center, with gentle preffure, flide it towards you till its edge be brought a little over that of the hones, then carry it quite acrofs the diameter as far the other fide, and having given the metal a light ftroke or two in this direction, take it off the tool. The metal being wiped quite dry, place it upon a table at a little diftance from a window; ftand yourfelf as near the window, at fome diftance from the metal, and looking obliquely on its furface, turn it round its axis, and you will fee at every half turn the grain given by the laft crofs ftrokes flafh upon your eye at once over the whole face of the metal. This is as certain a proof of a true fpherical figure as the operofe and difficult method defcribed in Dr. SMITH's Optics; for as there is nothing foft or elaftic, either in the metal or in the hones, this glare is a certain proof of a perfect contact in every part of the two furfaces; which there could not be if the fpheres were not both perfect and precifely the fame.

Indeed there is one accidental circumftance which neceffarily affords its aid in this and every bufinefs of the like fort; and that is, that a concave and convex furface

ground together, though ever ſo irregular at firſt, will (if the working be uniform and proper, conſiſting, eſpecially at laſt, of croſs ſtrokes in every poſſible direction acroſs the diameter) be formed into portions of true and equal ſpheres; had it not been for this lucky neceſſity, it would have been impoſſible to have produced that correctneſs which is eſſential in the ſpeculum of a good reflecting teleſcope by any mechanic contrivance whatever. For when it is conſidered, that the errors in reflection are four times as great as in refraction, and that the leaſt defect in figure is magnified by the powers of the inſtrument, any thing ſhort of perfection in the figure of the ſpeculum would be evidently perceived by a want of diſtinctneſs in the performance.

I muſt not, however, quit this article without obſerving, that I all along ſuppoſe, both in forming the tools and at laſt figuring the metal (and indeed the ſame muſt be obſerved in the future proceſs of poliſhing) that no kind of preſſure is uſed that may endanger the bending or irregularly grinding them; they ſhould therefore be held with a light hand, and looſely between the fingers, and the motion given ſhould be in a horizontal direction, with no more preſſure than their own dead weight.

Having now finiſhed the metal on the hones, and rendered it both in point of figure and ſurface fit for the

laſt

laſt and moſt eſſential proceſs, *viz.* that of poliſhing, I will deſcribe it in the beſt manner I can; though many little circumſtances which will be unavoidably omitted (and which at the ſame time are frequently eſſential to the ſucceſs of a mechanic proceſs) can only be ſupplied by actual experience.

The poliſhing the ſpeculum is the moſt difficult and eſſential part of the whole proceſs; for every experienced workman knows, to his vexation, that the moſt trifling error here will be ſufficient to ſpoil the figure of his metal, and render all his preceding caution uſeleſs. I have, however, diſcovered a method which I ſhall explain, not only of giving the metal a parabolic figure, but alſo of recovering it when it happens to be injured; both to be effected in the act of poliſhing, and the former as certainly as the ſpherical figure is given upon the hones. Indeed, if we conſider rightly, poliſhing will be perceived to be but a kind of grinding with a finer order of ſtrokes, and with a powder infinitely finer than was before uſed in what is commonly called the grinding. But before I deſcribe this method, which was the reſult of many years experience, I will take the liberty of making ſome few ſtrictures on that of Meſſ. HADLEY and MOLYNEUX, which is followed by the generality of workmen.

First, then, the tool itself used by them for polishing the metal, is formed with infinite difficulty. The first described polisher is directed to be made by covering the tool with sarcenet, which is to be saturated with a solution of pitch in spirit of wine, by successive applications of it with a brush, till it is covered, and by the evaporation of the spirit of wine filled with this extract of pitch; the surface is then to be worked down and finished with the bruiser. This is all very easy in imagination; but whoever has used this method (which I have myself unsuccessfully several times) must have found it attended with infinite labour, and at last the business done in a very unsatisfactory manner; for the pitch by this process will be deprived of an essential part of its composition. The spirit of wine dissolves none but the resinous parts of its substance, which is hard and untractable; and if you use soap or spirit of wine to soften or dissolve it, it will equally affect the whole surface, the lower as well as higher parts of it. And suppose that with infinite labour with the bruiser, it is at last reduced to a fine uniform surface, it is nevertheless too hard ever to give a good polish with that lustre which is always seen in Mr. SHORT's, and indeed all other good metals. Nor will it give a good spherical figure; for a perfect sphere is formed, as I observed before, by that intimate accommodation

dation arifing from the wear and yielding of both tool and metal; whereas in this method, there is fuch a ftubbornnefs in the polifher, that the figure of the metal, good or bad, muft depend upon the truth of the former, which is very feldom perfect.

If the polifher be made in the fecond manner propofed, by ftraining the pitch through an outer covering, which is afterwards to be ftripped off, the fuperficies of pitch and farcenet is fo very thin, that the putty, working into them, forms a furface hard and untractable, fo that it is impoffible to give the fpeculum a fine polifh. Accordingly all thofe metals which are wrought that way have an order of fcratches inftead of polifh, difcovering itfelf by a greyifh vifible furface. Befides, fuppofing this tool perfectly finifhed, and anfwering its purpofe ever fo well, it is impoffible it can produce in the fpeculum any other than a fpherical figure; and indeed nothing elfe is expected from this method, as very evidently appears by the experiment recommended to afcertain the truth of the figure. You are directed to place a fmall luminous object in the center of the fphere of which the metal is a fegment, and then having adjufted an eye-glafs at the diftance of its own focal length from the object, and fo fituated that the image of the object formed by the fpeculum may be vifible to the eye, you are to judge of the

 perfect-

perfect figure of the metal by the sharpness and distinctness with which the image appears. From hence it is very evident, that as the object and image are both distant from the metal by exactly its radius, nothing but a true spherical figure of the speculum can produce a sharp distinct image; and that the image could not be distinct if the figure of the speculum were parabolic. Consequently, if the same speculum used in a telescope were to receive parallel rays, there would necessarily be a considerable aberration produced, and a consequent imperfection in the image. Accordingly, there never was a good telescope made in this manner; for if the number of degrees, or the portion of the sphere of which the great metal is a part, were as considerable as it ought to be, or as great as Mr. SHORT allowed in his metal, the instrument would bear but a very low charge, unless a great part of the circumference of the metal were cut off by an aperture, and the ill effects of the aberration by that means in some measure prevented.

If ever a finished metal turned out without this defect, and has been found perfectly sharp and distinct, it must have been owing to an accidental parabolic tendency, no ways the natural result of the process, and therefore quite unexpected, and most probably unknown, to the workman.

Without

Without enlarging, therefore, on the difficulty of the above procefs, and the impoffibility of giving the fpeculum the correctnefs and the kind of figure effentially neceffary to a good telefcope, I will defcribe (by way of introduction to the fucceeding directions) the fteps by which I was led to a certain and eafy method of giving a proper and correct parabolic figure to the metal, even though it came off imperfect from the hones, and an exquifite polifh at the fame time.

Having made many efforts in the former method, which by no means pleafed me for the reafons above-mentioned; and having obferved, from fome of Mr. SHORT's telefcopes which fell into my hands, that the high luftre of the polifh could never have been produced in the manner above defcribed, but by fome fofter and more tender fubftance; and at the fame time recollecting, that Sir ISAAC NEWTON had given an account in his Optics of his having finifhed fome metals, and confiderably mended the object glafs of a refractor, by working both upon a tool whofe furface had been covered with common pitch about the thicknefs of a groat; reflecting, I fay, upon thefe matters (coarfe and uncertain as this method appeared at firft fight) I was determined to try whether I could not get rid of my embarraffment, by a mode of operation fomewhat fimilar. Accordingly, fhortening

Dr. SMITH's proceſs, I made a ſet of tools in the manner before deſcribed, except that I was obliged to make ſome ſubſequent alteration in the poliſher which I ſhall preſently deſcribe. Having given a good ſpherical figure to the braſs tool and the bruiſer, and likewiſe to the metal upon the hones, and made the braſs convex tool ſo hot as juſt not to hurt the finger, I tied a lump of common pitch (which ſhould be neither too hard nor too ſoft) in a rag, and holding it in a pair of tongs over a ſtill fire where there was no riſing duſt, till it was ready to ſtrain through the linen, I cauſed it to drop upon the ſeveral parts of the convex tool, till I ſuppoſed it would cover the whole ſurface about double the thickneſs of a ſhilling; then ſpreading the pitch as equally as I could, I ſuffered the poliſher (by which name I ſhall for the future call this tool) to grow quite cold. I then warmed the bruiſer ſo hot as almoſt to burn my fingers, and having fixed it to the bench with its face upwards, I ſuddenly placed the poliſher upon it, and quickly ſlid it off; by this means rendering the ſurface of the pitch more equal. The pitch is then to be wiped off from the bruiſer with a little tow; and by touching the ſurface with a tallow candle, and wiping it a ſecond time, it will be then perfectly clean and fit for a ſecond proceſs of the ſame ſort, which muſt again be performed as quickly as poſſible;

and

and this is ordinarily fufficient to give a general figure to the furface of the pitch. The bruifer and polifher are then fuffered to grow perfectly cold, when the pitch, confidering what has been taken off, will be about the thicknefs of a fhilling.

It is however here neceffary to obferve, that the pitch fhould be neither very hard and refinous, nor too foft; if the former, it will be fo untractable as not to work kindly; and if too foft, it will in working alter its figure fafter than the metal, and too readily fit itfelf to the irregularity of its figure, if it have any. When both tools were perfectly cold, I gave the polifher a gentle warmth, and then fixed the bruifer to the block with its face upwards; and (having with a large camel's-hair brufh fpread over the face of the polifher a little water and foap, to prevent fticking) with fhort, ftraight, and round ftrokes I worked it upon the bruifer, every now and then adding a little more water and foap, till the pitch upon the polifher had a fine furface, and the true form of the bruifer; and this I continued to do till they both grew perfectly cold together: in this manner the polifher was perfectly formed in about a quarter of an hour. But here a difficulty arofe: when I begun to polifh the metal, I found that the edge of the hole in the metal collected the pitch towards the middle of the polifher; and

though in this method of working I could give an exquiſite poliſh, as the putty lodged itſelf in the pitch exceedingly well, yet the figure of the metal was injured in the middle, nor did indeed the work go on with that equability which is the inſeparable attendant on a good figure. In order to obviate this difficulty, I caſt ſome metals with a continued face, the holes not going quite through, within perhaps the thickneſs of a ſix-pence. I finiſhed two or three metals of this ſort, and the work promiſed and went on very well; but when I came to open the holes, which I did with the utmoſt caution, I found the metals ſhort of perfection; which I attributed to an alteration of the figure from the removal of even that ſmall portion of metal after the ſpeculum had been finiſhed. This I do ſuppoſe was in ſome meaſure the reaſon why I ſpoiled a very diſtinct and perfect two-foot metal, which bore a charge of two hundred times, only by opening the ſharp part of the edge of the hole, becauſe I thought it bounded the field: ſo eſſentially neceſſary is an exquiſite correctneſs of figure in the ſpeculum of a perfect reflector.

This experiment not ſucceeding, inſtead of caſting the metal without a hole, I made one quite through the middle of the poliſher, a little leſs than that in the ſpeculum. This perfectly anſwered the purpoſe; no more incon-

inconvenience arofe from the gathering of the pitch (for it had now no greater tendency to collect at the center than the fides) and I finifhed feveral metals fucceffively, excellent both in point of figure and polifh; one of thofe of two inches diameter and 7,5 focal length, bore a charge of fixty times and upwards, which when mounted in a telefcope I gave to my brother. This telefcope underwent Mr. SHORT's examination, who was pleafed to remark only, that he thought he had made one more diftinct.

I muft obferve, that in this method of working the polifhing goes on in an agreeable, uniform, and fmooth manner; and that the fmall degree of yielding in the pitch (which is actually not more than the wearing of the metal) produces that mutual accommodation of furfaces fo neceffary to a true figure. In the beginning of the polifh, and indeed for fome time during the progrefs of it (always remembering now and then to move the metal round its axis) I worked round and round, not far from and always equally diftant from the center, except that every time, previous to the fhifting the metal on its axis, I ufed a crofs ftroke or two; and when the polifh was nearly compleated, I moftly ufed crofs ftrokes, giving a round ftroke or two likewife every time I turned the metal on its axis. I obferved in this method of working, that the metal always polifhed fafteft in the middle; infomuch,

inſomuch, that half or two-thirds of it would be completely poliſhed when the circumference of it was ſcarcely touched by the tool. Obſerving this in ſome of the firſt metals, and not conſidering that this way of poliſhing was in fact a ſpecies of grinding, and as perfect as that upon the hones, I went on reluctantly with the work, almoſt deſpairing of being able to produce a good figure. However, I always found myſelf agreeably deceived; for when the poliſh was extended to the edge, or within the tenth of an inch of it, I almoſt conſtantly found the figure good, and the performance of the metal very diſtinct. But this ſame circumſtance of apparent defect in the metals, was in fact that to which their perfection was owing; for they all, contrary to my expectation, turned out parabolic. However I did not for a great while know any certain way of giving that degree of parabolic tendency which was juſt neceſſary, and which will be deſcribed hereafter. It was a long time before I got rid of my prejudice againſt this apparent imperfection in the proceſs, or could reconcile myſelf to the irregular manner in which the poliſh proceeded; for I looked upon it as a certain ſource of error, and notwithſtanding I ſaw it eventually ſucceed, yet whenever I chanced to find that a metal, when firſt applied to the poliſher, took the poliſh equally all over,

and

and confequently the whole bufinefs did not take up above ten minutes; under thofe circumftances, I fay, I always ufed to pleafe myfelf with the expectation of a correct figure, at leaft as much fo as the metal had received from the hones, where the furface was but juft and equally taken off by the putty; but in this I conftantly found myfelf deceived, and the metal turned out good for nothing. In fhort, at this time, though I fpeculatively knew that a parabolic figure was neceffary to a perfect image, I yet confidered it as of little practical confequence.

From the foregoing experiments, and a number of fucceeding trials, I at length difcovered a certain way of giving a correct parabolic figure, and an exquifite polifh at the fame time. This, which I have ftrong reafons to believe was Mr. SHORT's method, I will now defcribe in as few words as I can.

How to polifh the fpeculum.

It is firft neceffary to obferve, that, in order to avoid the detrimental intrufions of any particles of emery, it would not be right to polifh in the fame room where the metal and tools were ground, nor in the fame cloaths which were worn in the former procefs; at leaft it would

be neceſſary to keep the bench quite wet, to prevent any duſt from riſing.

Having then made the poliſher by coating the braſs convex tool equally with pitch, which we ſuppoſe ſmoothed and finiſhed with the braſs tool in the manner before deſcribed, and which is a very eaſy proceſs, the whole operation is begun and finiſhed in the following manner.

The leaden weight or handle upon the back of the metal ſhould be divided into eight parts, by ſo many deep ſtrokes of a graver upon the upper ſurface of the lead, marking each ſtroke with the numbers 1, 2, 3, 4, and ſo on, that the turns of the metal in the hand may be known to be uniform and regular.

To prevent any miſchief from coarſe particles of putty, I always waſh it immediately before uſing. In order to this, put about half an ounce of putty into an ounce phial, and fill it two-thirds with water; then having ſhaken the whole, let the putty ſubſide, and ſtop the bottle with a cork.

In a tea-cup with a little water, there ſhould be a full-ſized camel's-hair bruſh, and a piece of dry clean ſoap in a galley-pot: a ſoft piece of ſpunge will alſo be neceſſary. Theſe, as well as the metal bruiſer and poliſher, ſhould be conſtantly covered from duſt.

The

The polisher being fixed down, and the camel's-hair brush, being first wetted and rubbed a little over the soap, let every part of the tool be brushed over therewith; then work the bruiser with short, straight, and round strokes, lightly upon the tool, and continue to do so, now and then turning it, till the polisher have a good face, and be fit for the metal. Then having shaken up the putty in the phial, and touched the polisher in five or six places with the cork wetted with that and the water, place the bruiser upon the tool, and give a few strokes upon the putty to rub down any gritty particles; after which, having removed it, work the metal lightly upon the polisher round and round, carrying the edges of the speculum, however, not quite half an inch over the edge of the tool, and now and then with a cross stroke.

The first putty, and indeed all the succeeding applications of it, should be wrought with a considerable while; for if time be not given for the putty to bed itself in the pitch, and any quantity of it lie loose upon the polisher, it will accumulate into knobs, which will injure the figure of the metal: and therefore as often as ever such knobs arise, they must be carefully scraped off with the point of a penknife, and the loose stuff taken away with the brush. After the putty is well wrought into the pitch, some more may be added in the same manner, but

 never

never much at a time, and always remembering to work upon it firſt with the bruiſer, for fear any gritty particles may find their way upon the poliſher. If the bruiſer be apt to ſtick, and do not ſlide ſmoothly upon the pitch, the ſurface of either tool may be occaſionally bruſhed over with the ſoap and water, but it muſt be remembered that the wet bruſh muſt be but lightly rubbed upon the ſoap.

In the beginning of this proceſs little effect is produced, and the metal does not ſeem to poliſh faſt, in ſome meaſure owing to its taking the poliſh in the middle, and perhaps becauſe neither that nor the bruiſer move evenly upon the poliſher: but a little perſeverance will bring the whole into a good temper of working; and, when the pitch is well defended by the coating of the putty, the proceſs will advance apace, and the former acquiring poſſibly ſome little warmth, the metal moves more agreeably over it, with an uniform and regular friction. All this while the metal muſt have no more preſſure than that which it derives from its own weight and that of the handle; and the poliſher muſt never be ſuffered to grow dry, but, as often as it has any tendency to do ſo, the edges of it muſt be moiſtened with the hair-pencil; and now and then, even when freſh putty is not laid on, the ſurface of the poliſher ſhould be touched with the bruſh to keep it moiſt.

When the poliſh of the metal nearly reaches the edge (for it always, as I ſaid before, begins in the middle) you muſt alter your method of working; for now the round ſtrokes muſt be gradually altered for the ſhort and ſtraight ones. Suppoſing then you are juſt beginning to alter them; after having put on freſh putty, and gently rubbed it with two or three ſtrokes of the bruiſer, you place the metal on the tool, and after a ſtroke or two round and round, give it a few forward and backward, and from ſide to ſide, but with the edges very little over the tool; then having turned the metal one-eighth round in your hand, and having moved yourſelf as much round the block (which muſt be remembered throughout the whole proceſs) you go on again with a ſtroke or two round, to lead you only to the croſs ſtrokes, which are now to be principally uſed, and with more boldneſs. After this has been done ſome time, the metal will begin to move ſtiffly as the friction now increaſes, and the ſpeculum poliſhes very beautifully and faſt; and the whole ſurface of the poliſhing tool will be equally covered over with a fine metallic bronze. The tool even now muſt not be ſuffered to become dry; a ſingle round ſtroke in each of your ſtations and turnings of the metal will be ſufficient, and the reſt muſt all be croſs ones, for we are completing a circular figure. You muſt now be very

 diligent,

diligent, for the poliſher drying, and the friction increaſing very faſt, the buſineſs of the ſpherical figure is nearly at an end. As the metal wears much, its ſurface muſt be now and then cleaned, with a piece of ſhammy leather, from the black ſtuff which collects upon it; and the poliſher likewiſe from the ſame matter, with a ſoft piece of wet ſponge. You will now be able to judge of the perfect ſpherical figure of the metal and tool, when there is a perfect correſpondence between the ſurfaces, by the fine equable feel there is in working, which is totally free from all jerks and inequalities. Having proceeded thus far, you may put the laſt finiſhing to this figure of the metal by bold croſs ſtrokes, only three or four in the directions of each of the eight diameters, turning the metal at the ſame time: this muſt be done quickly, for it ought, in this part of the proceſs particularly, to be remembered, that, if you permit the tool to grow quite dry, you will never be able, with all your force, to ſeparate that and the metal, without deſtroying the poliſher by heat.

The metal has now a beautiful poliſh and a true ſpherical figure, but will by no means make a ſharp diſtinct image in the teleſcope: for the ſpeculum (if it be tried in the manner hereafter recommended) will not be found to make parallel rays converge without great aberration;

aberration; indeed the deviation will be ſo great, as to be very ſenſibly perceived by a great indiſtinctneſs in the image.

How to give the parabolic figure to the metal.

In order then to give the ſpeculum the laſt and finiſhing figure, which is done by a few ſtrokes, it muſt be particularly remarked, that by working the metal round and round, the ſphere of the poliſher by this means growing leſs, it wears faſteſt in the middle: and as a ſegment of a ſphere may become parabolic, by opening the extremes gradually from within outwards, ſo it may be equally well done by increaſing the curvature in the middle, in a certain ratio, from without inwards.

Suppoſing then the metal to be now truly ſpherical, ſtop the hole in the poliſher, by forcing a cork into it underneath, about an inch, ſo that it do not reach quite to the ſurface; and having waſhed off any mud that may be on the ſurface of the tool with a wet ſoft piece of ſponge, whilſt the ſurface of it is a little moiſt, place the center of the metal upon the middle of the poliſher; then having, with the wet bruſh, lodged as much water round the edge of the metal as the projecting edge will hold,

hold, fill the hole of the metal and its handle with water, to prevent the evaporation of the moiſture, and the conſequent adheſion between the ſpeculum and poliſher, and let the whole reſt in this ſtate two or three hours: this will produce an intimate contact between the two, and by parting with any degree of warmth they may have acquired by the vicinity of the operator, they will grow perfectly cold together.

By this time you may puſh out the cork from the poliſher, to diſcharge the water, and give the metal the parabolic figure in the following manner.

Move the metal gently and ſlowly at firſt, a very little round the centre of the poliſher (indeed after this reſt it will move ſtiffly) then increaſing by degrees the diameter of theſe ſtrokes, and turning the metal frequently round its axis, give it a larger circular motion, and this without any preſſure but its own weight, and holding it looſely between the fingers: this manner of working may ſafely be continued about two minutes, moving yourſelf as uſual round the block, and carrying the round ſtrokes in their increaſed and largeſt ſtate, not more than will move the edge of the metal half an inch or five-eighths over the tool. The ſpeculum muſt not all this while be taken off from the poliſher; and conſequently no freſh putty can be added.

added. It will not be ſafe to continue this motion longer than the time above-mentioned; for if the parabolic tendency be carried the leaſt too far, it will be impoſſible to recover a true figure of that kind but by going through the whole proceſs for the ſpherical one in the manner before deſcribed, by the croſs ſtrokes upon the poliſher, which takes a great deal of time. However, when there is occaſion, it may be done; and I have myſelf ſeveral times recovered the circular figure, when I had inadvertently gone too far with the parabolic; and ultimately finiſhed the metal on the poliſher without the uſe of the hones.

To try the true figure of the metal.

It will now be proper to try the figure of the ſpeculum, and that is always beſt done by placing it in the teleſcope it is intended for. In order to this, I uſe the inſtrument as a kind of microſcope, placing the object, however, at ſuch a diſtance that the rays may be nearly parallel. At about twenty yards a watch-paper, or ſome ſuch object, on which there are ſome very fine hair ſtrokes of a graver, is fixed up. The lead muſt be then taken off from the back of the ſpeculum; which is beſt done by placing the edge of a knife at the junction of the lead and metal, when, by ſtriking the back of it with a

ſlight blow, the pitch immediately ſeparates, and the handle drops off; the remaining pitch may be ſcraped off with a knife, taking care that none of the duſt ſtick to the poliſhed face of the metal.

Having placed the ſpeculum in the cell of the tube, and directed the inſtrument to the object, make an annular kind of diaphragm with card-paper, ſo as to cover a circular portion of the middle part of the metal between the hole and the circumference, equal in breadth to about an eighth part of the diameter of the ſpeculum: this paper ring ſhould be fixed in the mouth of the teleſcope, and remain ſo during the whole experiment, for the part of the metal covered by it is ſuppoſed to be perfect, and therefore unemployed.

There muſt likewiſe be two other circular pieces of card-paper cut out, of ſuch ſizes, that one may cover the center of the metal by completely filling the hole in the laſt deſcribed annular piece; and the other, ſuch a round piece as ſhall exactly fit into the tube, and ſo broad as that the inner edge may juſt touch the outward circumference of the middle annular piece. It would be convenient to have theſe two laſt pieces ſo fixed to an axis that they may be put in their places, or removed from thence ſo eaſily as not to diſplace or ſhake the

 inſtrument.

inſtrument. All theſe pieces therefore together will completely ſhut up the mouth of the teleſcope.

Let the round piece which covers the center of the metal, or that which has no hole in it, be removed; and, by a nice adjuſtment of the ſcrew, let the image (which is now formed by the center of the mirror) be made as ſharp and diſtinct as poſſible. This being done, every thing elſe remaining at reſt, replace the central piece, and remove the outſide annular one, by which means the circumference only of the ſpeculum will be expoſed, and the image now formed will be from the rays reflected from the outſide of the metal. If there be no occaſion to move the ſcrew and little metal, and the two images formed by theſe two portions of the metal be perfectly ſharp and equally diſtinct, the ſpeculum is perfect, and of the true parabolic curve; or at leaſt the errors of the great and little ſpeculum, if there be any, are corrected by each other.

If, on the contrary, under the laſt circumſtance, the image from the outſide of the metal ſhould not be diſtinct, and it ſhould become neceſſary, in order to make it ſo, that the little ſpeculum be brought nearer, it is plain that the metal is not yet brought to the parabolic figure; but if, on the other hand, in order to procure diſtinctneſs, you be obliged to move the little ſpeculum farther off, then the figure of the great ſpeculum has been carried beyond

the parabolic, and hath aſſumed an hyperbolic form. When the latter is the caſe, the circular figure of the metal muſt be recovered (after having fixed on the handle with ſoft pitch) by bold croſs ſtrokes upon the poliſher, finiſhing it again in the manner above deſcribed. If the ſpeculum be not yet brought to the parabolic form, it muſt cautiouſly have a few more round ſtrokes upon the poliſher; indeed a very few of them in the manner before deſcribed make in effect a greater difference in the ſpeculum than would be at firſt imagined. If a metal of a true ſpherical figure were to be tried in the above mentioned manner in the teleſcope (which I have frequently done) the difference of the foci of the two ſegments of the metal would be ſo conſiderable, as to require two or three turns of the ſcrew to adjuſt them; ſo very great is the aberration of a ſpherical figure of the ſpeculum, and ſo improper to procure that ſharpneſs and preciſion ſo neceſſary to a good reflecting teleſcope.

This is by no means the caſe with the object glaſſes of refractors; for beſides that they are in fact never ſo diſtinct as well-finiſhed reflectors, the apertures of them are ſo exceedingly ſmall, compared to the latter, and the number of degrees employed ſo very ſmall, that the inconvenience of a ſpherical figure is not ſo much perceived. Accordingly we obſerve in the generality of

 reflectors

reflectors (whose specula, unless by accident, are always spherical) that the only true rays which form the distinct image arise from the middle of the metal: and unless the defect be remedied by a considerable aperture, which destroys much light, the false reflection from the inside of the metal produces a greyish kind of haziness, which is never seen in Mr. SHORT's or indeed in any good telescopes.

Supposing that the two foci of the different parts of the metal perfectly coincide, and that, by the union of them when the apertures are removed, the telescope shews the objects very sharp and distinct, you are not however even then to conclude that the instrument is not capable of farther improvement; for you will perceive a sensible difference in the sharpness of the image, under different positions of the great speculum with respect to the little one, by turning round the great metal in its cell, and opposing different parts of it to different parts of the little metal, correcting by this means the error of one by the other. This attempt should be persevered in for some time, turning round the great speculum about one-sixteenth at a time, and carefully observing the most distinct situation each time the eye-piece is screwed on: when, by trying and turning the great metal all round, the distinctest position is discovered, the upper part of the

 metal

metal ſhould be marked with a black ſtroke, in order that it may always be lodged in the cell in the ſame poſition. This is the method Mr. SHORT always uſed; and the caution is of ſo much conſequence, that he thought it neceſſary to mention it very particularly in his printed directions for the uſe of the inſtrument.

And farther, Mr. SHORT frequently corrected the errors of the great by the little metal in another way. If the great ſpeculum did not anſwer quite well in the teleſcope, he cured that defect ſometimes by trying the effect of ſeveral metals ſucceſſively, by this means correcting the errors of one by the other; for in ſeveral of his teleſcopes which have paſſed through my hands, when the ſizes and powers have been the ſame, I have found that the great metals, though very diſtinct in their proper teleſcopes, yet have, when taken out and changed from one to the other, ſpoiled both teleſcopes, rendering them exceedingly indiſtinct, which could ariſe from no other circumſtance. For this reaſon I ſuppoſe it was, that he kept, ready finiſhed, a great many large metals of the ſame focal length, ſo that, when he wanted to mount a teleſcope, he might from a great choice, be able to combine thoſe metals which ſuited each other beſt. I am ſtrongly inclined to believe this was the caſe, not only from the above obſervation, but becauſe

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he ſhewed me himſelf a box of finiſhed metals, in which I am ſure there were a dozen and a half of the ſame focal length.

To return: a little uſe in working will make the whole of the proceſs of grinding and poliſhing very eaſy and certain; for though I have endeavoured to be as particular as I can (I am almoſt afraid too much ſo) it is yet ſcarcely poſſible to ſupply a want of dexterity, ariſing from habit only, by the moſt laboured and minute deſcription. And though the above account may appear irkſome to the reader, as it lies cold before the eye, I am very ſure, whoever attempts to make the inſtrument, will not complain of it as tediouſly particular.

I will, however, farther remark, that when the metal begins to move ſtiffly upon the poliſher, and particularly when the figure is almoſt brought to the parabolic form, it will be neceſſary to fix the elbows againſt the ſides, in order to give momentum and equability to the motion of the hand by that of the whole body.

The ſame poliſher will ſerve for ſeveral metals, if it be ſomewhat warmed when you begin to uſe it.

There is another circumſtance, and a material one too, which muſt not be omitted; it is this. For the very ſame reaſon that the pitch ſhould not be too hard or ſoft, the work will not proceed well in the heat of ſummer,

or the cold of winter: in the latter, it may be poſſible to remedy the defect by having the room warmed with a ſtove; and in the ſummer, the other inconvenience may perhaps be avoided by uſing a harder kind of pitch; but I much doubt in either caſe whether the work will go on ſo kindly: I have myſelf always wrought in ſpring and autumn.

The proceſs of poliſhing, and indeed grinding upon the hones, will not go on ſo well if it be not continued uninterruptedly from beginning to end; for if the work of either kind be left but for a quarter of an hour, and you then return to it again, it will be ſome time before the tool and metal can get into a kindly way of working; and till they do, you are hurting what was done before.

I have all along ſuppoſed that the metal we have been working was about four inches diameter: if it be either larger or ſmaller, the ſizes of the hones, bruiſer, and poliſher, muſt be proportionably different. I never find any ill conſequence ariſing from the different expanſion from heat and cold in any of the tools, though they be made of different metals and ſubſtances, unleſs the inconvenience, occaſioned by the interruption before hinted at, be thought to reſult from thence; for the alteration produced in the ſurface of the ſpeculum, both by grinding and poliſhing, is ſo much quicker than any that can be ſuppoſed to ariſe

from

from the former cauſe, that it is never attended with any practical conſequence.

Magnifying very minute objects, and particularly reading at a diſtance, have been generally conſidered as the ſureſt teſts of the goodneſs of a teleſcope; and indeed when the page is placed at a great diſtance, ſo that the letters ſubtend but a very ſmall angle at the eye, if then they appear with great preciſion and ſharpneſs, it is moſt probable that the inſtrument is a good one. But we are, nevertheleſs, ſometimes apt to be deceived by this method; nor is it always poſſible to determine upon the different merits of two inſtruments of equal power, by this mode of examination; for when the letters are removed to the utmoſt extent of the powers of the two inſtruments, the eye is apt to be prejudiced by the imagination. If two or three words can be here and there made out, all the reſt are gueſſed at by the ſenſe; inſomuch that an obſerver, zealous for the honour of his inſtrument, is very apt to deceive himſelf in ſpite of his intentions. The ſurer teſt is by figures, where you can procure no aid from this ſort of deception. In order to examine my reflecting teleſcopes, I made upon a piece of copper and on a black ground, ſix lines conſiſting of about twelve pieces of gold figures, and each line of figures differing in magnitude, from the ſmalleſt that could be diſtinctly made to thoſe

of

of about two-tenths of an inch long; moreover, the figures in the ſeveral lines were differently diſpoſed, and the ſum of each line alſo differed. It is evident that by this method all gueſs is precluded; and that of two inſtruments, of the ſame powers, that which can make out the leaſt order of figures, which will be known by the ſum, is the beſt teleſcope. Such a plate I cauſed to be fixed up for experiments againſt the top of a ſteeple, about three hundred yards North of my houſe; and it will ſerve to give ſome idea of the diſtinctneſs with which very ſmall figures could be made out at that diſtance, by ſaying, that in a clear ſtate of the air, and with the Sun behind me, with a teleſcope of eighteen inches focal length, which Count BRUHL did me the honour to accept and now has in his poſſeſſion, I have ſeen the legs of a ſmall fly, and the ſhadows of them, with great preciſion and exactneſs.

I cannot conclude without indulging myſelf in an obſervation on the amazing ſagacity of Sir ISAAC NEWTON in every ſubject upon which he thought fit to employ his attention. It was he who firſt propoſed, and indeed practiſed, the poliſhing with pitch; a ſubſtance which at firſt ſight perhaps every one but himſelf would have thought very improper, from its ſoftneſs, to produce that correctneſs of figure ſo neceſſary upon theſe occaſions; and yet I do believe, that it is the only ſubſtance in nature

2 that

that is perfectly well calculated for the purpofe; for at the fame time that it is foft enough to fuffer the putty to lodge very freely on its furface, and for that reafon to give a moft tender and delicate polifh; it is likewife totally inelaftic, and therefore never, from that principle, fuffers any alteration in the figure you give it. If the firft makers of the inftrument, therefore, had given proper credit to, or had fimply followed the hint Sir ISAAC gave, it would have faved them infinite trouble, and they would have produced much better inftruments; but the pretended refinement, of drawing a tincture from pitch with fpirits of wine, affords you only the refinous, hard, and untractable part of the pitch, divefted of all that part of its original fubftance which is neceffary to give it that accommodating pliability in which its excellence confifts.

It is needlefs to fwell this account with a detail of the procefs for polifhing the little fpeculum, as it muft be conducted in the fame manner which has been already defcribed in that of the large one; only obferving, that as the little metal has an uninterrupted face, without a hole, fo there is no occafion for one in the polifher; and likewife that, as a fpherical figure is all that need here be practically attempted, fo the difficulty in finifhing is infinitely fhort of that of the other.

 As

As it is always neceſſary to ſolder to the back of the little ſpeculum a piece of braſs, as a fixture for the ſcrew to adjuſt its axis, I ſhall juſt hint a ſafe and neat method of doing it, which may be very uſeful to the optical or mathematical inſtrument-maker upon other occaſions. Having cleaned the parts to be ſoldered very well, cut out a piece of tin-foil the exact ſize of them; then dip a feather into a pretty ſtrong ſolution of *ſal ammoniac* in water, and rub it over the ſurfaces to be ſoldered; after which place the tin-foil between them as faſt as you can (for the air will quickly corrode their ſurfaces ſo as to prevent the ſolder taking) and give the whole a gradual and ſufficient heat to melt the tin. If the joints to be ſoldered have been made very flat, they will not be thicker than a hair: though the ſurfaces be ever ſo extenſive, the ſoldering may be conducted in the ſame manner, only that care muſt be taken, by general preſſure, to keep them cloſe together. In this manner, for inſtance, a ſilver graduated plate may be ſoldered on to the braſs limb of a quadrant, ſo as not to be diſcernable by any thing but the different colour of the metals. This method was communicated to me by the late Mr. JACKSON, who during his life kept it a ſecret, as he uſed it in the conſtruction of his quadrants, and is, I believe, not as yet known to any workman.

In

In the annexed plate are figured the ſhape of the leaden tool for rough-grinding; the hones; and the apparatus to be applied to the mouth of the teleſcope, to aſcertain the true figure of the ſpeculum.

P O S T S C R I P T.

It was ſome time after I had written the above account that I ſaw Mr. SHORT's method of poliſhing object glaſſes for refracting teleſcopes, which is publiſhed in the Tranſactions. By that paper I find that what I before ſtrongly ſuſpected is really the caſe, *viz.* that he knew how well pitch was calculated for purpoſes of this kind. Only it may be remarked, that as glaſs is much harder, poliſhes much ſlower, and conſequently does not wear away and alter its figure ſo ſoon as the metal of which the ſpeculum is made; and as at the ſame time (on account of the very ſmall apertures allowed to teleſcopes of this ſort) nothing more than a ſpherical figure is propoſed; he is therefore obliged to uſe pitch in a hard, friable, and ſtubborn ſtate: whereas, conſidering the delicate ſubſtance of the metal ſpeculum, and the figure intended to be given to it, the ſoft pitch of the common ſort, by ſuffering the putty to bed itſelf in its ſubſtance,

produces the moſt beautiful poliſh; and by its pliability is better calculated for that mutual accommodation between poliſher and metal, ſo neceſſary to the figure propoſed.

EXPLANATION OF THE FIGURES.

Fig. 1. The grinder for working off the rough face of the metal; the black ſtrokes repreſent deep grooves made with a graver.

Fig. 2. The bed of hones, which is to complete the ſpherical figure of the ſpeculum, and to render its ſurface fit for the poliſher.

Fig. 3. An apparatus for examining the parabolic figure of the ſpeculum.

AA The mouth of the teleſcope, or edge of the great tube.

BB A thin piece of wood faſtened into, and fluſh with the end of the tube; to which is permanently fixed the annular piece of paſte-board cc, intended to cover, and to prevent the action of the correſponding part of the ſpeculum.

Fig: 1st

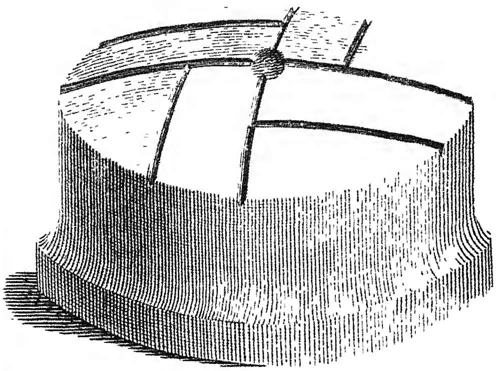


Fig: 2d

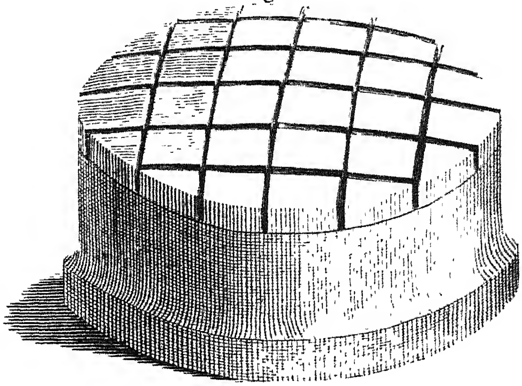
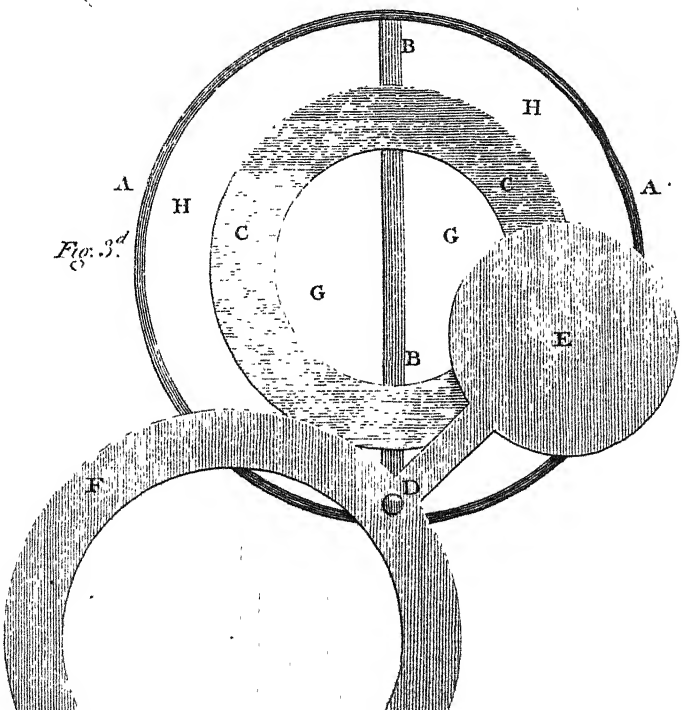


Fig: 3d



D Another piece of paſte-board, fixed by a pin to the piece of wood BB, on which it turns as on a center; ſo that the great annular opening HH may be ſhut up by the ring FF, or the aperture GG by the imperforate piece E in ſuch manner that, in the firſt inſtance, the reflexion may be from the center, and in the latter from the circumference, of the great ſpeculum.

XVII. *Extract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in Rutland,* 1776. *By* Thomas Barker, *Esquire. Communicated by Sir* John Pringle, *Bart. P. R. S.*

Read February 20, 1776.

		Barometer.			Thermometer.						Rain.
					In the House.			Abroad.			
		Highest	Lowest.	Mean.	High.	Low.	Mean	High.	Low.	Mean	
Jan.	Morn.	29,65	28,88	29,27	42½	23	32½	42	10	26	2,511
	Aftern.				43	26	33	45	16	29	
Feb.	Morn.	29,50	28,24	28,89	44½	24	39	44	11	35	3,195
	Aftern				45	25	40	45½	27	41½	
Mar.	Morn.	29,95	28,50	29,46	53½	38½	44½	47½	27	38	1,518
	Aftern.				55	40	46	62½	37	48½	
Apr.	Morn.	29,94	29,00	29,58	54½	43	50	52	31	42½	0,887
	Aftern.				57½	44	51½	64½	42	55	
May	Morn.	29,99	28,73	29,57	62½	47	51½	58	38	47	1,627
	Aftern.				66	48	53	76	38	57	
June	Morn.	29,87	29,04	29,42	66	55½	59	62	48½	54	2,485
	Aftern.				69½	57½	61	75	54	64½	
July	Morn.	29,86	29,03	29,45	65	57	62	63½	53	58	1,850
	Aftern.				68½	59½	64	80	65	69½	
Aug.	Morn.	29,83	29,00	29,41	70	57	61	63½	46	54	5,200
	Aftern.				75	59	62½	82½	58½	66	
Sept.	Morn.	29,93	28,75	29,40	62	52½	57½	60	37	49½	2,452
	Aftern.				64	54½	58½	69	52½	61	
Oct.	Morn.	29,87	29,06	29,56	57½	48	52½	53	36	45½	2,061
	Aftern.				58	48	53½	62	48	54	
Nov.	Morn.	29,85	28,60	29,42	52	37	45½	54	26½	38½	2,823
	Aftern.				52	37	46	54	32½	44	
Dec.	Morn.	29,94	28,73	29,43	48½	34	42½	49	21½	37	1,233
	Aftern.				49	34	43	52	26½	40½	
											27,842

The

The year began wet, but there ſoon fell a greater quantity of ſnow than for ſeveral years paſt: we had perhaps the ſharpeſt froſt ſince 1740, and it was more intenſe at the latter part of it than at the beginning. The froſt went away finely the beginning of February, and without much rain till the ſnow was almoſt gone; but a good deal of the middle of February was ſtormy and wet, and it was chiefly wet till about ten days in March, yet not cold. Then the ſeaſon grew dry; the ſeed-time was fine, pleaſant, growing warmer, and in the middle of April hot. There was great plenty of bloſſoms of all ſorts, and the graſs came on well; but the wheat, which had been left thin by the great froſt and ſnow, was rather hurried on too faſt. It grew colder at the end of April, and was cool and dry moſt part of May, with chiefly Northerly winds. During this time the wheat mended much, but rain began to be wanted, of which there came ſome in June, and brought on both corn and graſs. The latter end of that month and July were only ſhowery and hotter, ſo that the hay was got in well; and toward the end of July and beginning of Auguſt, the ground began to burn pretty much, when after ſome very hot days there came a great deal of wet.

The courſe of the ſeaſons this year was I believe the ſame in all places. Dry ſpring and beginning of ſummer, ſhowery June, drier July, very hot beginning of Auguſt,

and wet after; but in different proportions in various places. In fome, as Leicefterfhire and Northamptonfhire, the drought fo much prevailed that the ground was greatly burnt, and hay very fcarce; on the other hand here and in Huntingdonfhire, there were fo many refrefhing fhowers that we never were in want of grafs.

The beginning of harveft was wet, and the reft fhowery. I believe a little of the wheat might grow, but in general the grain was pretty well got in. It was a remarkably plentiful year for almoft all kinds of fruit; the crop of grain was pretty good, efpecially the barley; and there were great quantities of latter grafs and turnips. In the middle of September the weather grew fairer; it was a fine latter end of the year and wheat feed-time, without too much rain intermixed. As the winter came on, it was chiefly calm, and there was much cloudy or mifty weather, fcarce any frofty mornings till near the end of November; a fhort froft then and mild again; but towards the latter half of December it began to be more inclined to froft, and the year ended with a fharp one, and pretty deep fnow.

XVIII. *Extract of a Meteorological Journal for the Year* 1776, *kept at* Bristol, *by* Samuel Farr, *M. D.*

Read February 27, 1777.

Months.	Barometer. Highest.	Lowest.	Mean.	Vicissitude.
January	29,93	29,04	29,53	— 47– ½
February	29,74	28,66	29,21	+ 102–2
March	30,28	28,80	29,53	+ 110–2
April	30,28	29,26	29,85	+ 68–2
May	30,30	29,10	29,88	— 40– ½
June	30,20	29,38	29,88	— 39–1½
July	30,16	29,30	29,76	— 40–1½
August	30,08	29,30	29,68	— 30– ½
September	30,20	20,17	29,72	— 98–4
October	30,18	29,60	29,87	+ 54–1
November	30,15	28,90	29,74	+ 65– ½
December	30,28	29,26	29,76	+ 32– ½
				+ rising. — falling.

An

An abridged Table of the WINDS, &c. for BRISTOL, for the Year 1775.

	N	E	W	S	NW	SE	NE	SW	Rain.	Frosty Days.	Fair Days.	Thunder, &c,
Jan.	0	1	0	0	½	3	23½	3	3,993	25	6	
Feb.	0	0	0	1½	1	3½	2½	20½	5,538	1	5½	5. S.W.
Mar	0	1½	0	1½	1½	3	7½	16	1,643	2½	13½	
Apr.	1½	1	½	2	4	2	10½	8½	0,438	1	18	
May	2½	2	½	2½	4	1	11½	7	1,149	0	13½	
June	½	½	0	3	2	5	5½	13½	2,554	0	13½	13. S.
July	0	0	½	3	3	5	1	18½	2,332	0	13	5. S.E. 19. S.E.
Aug.	½	½	½	1	5	6½	3	14	4,747	1	11½	6. N.W. 16. S.W. 30. S.W.
Sept.	0	0	0	½	4	3	10	12½	3,270	5	15½	4. N.W. 8. N.W.
Oct	0	1½	0	1½	3½	4	13	7½	1,686	4	6	
Nov.	1	0	½	0	2	13	1½	12	2,283	5	10½	30. S.E.
Dec.	3½	½	½	½	3½	10½	2	10	1,422	9	11	1. S.E.
	9½	8½	3	17	34	59½	91½	143	31,055	53½	137½	

WEATHER FOR THE YEAR 1776.

January. It rained a little at firft; but foon changed to fnow, which was fucceeded by a hard froft that lafted till the next month.

February. After the thaw it became very wet and tempeftuous.

March. Often frofty and fair, with now and then a confiderable ftorm, as on the 8th and 9th.

April. Stormy, although but little rain fell for about half this month; after the 20th it was quite fair.

May. After this it was ftormy, with fome rain on the 4th, 5th, 6th, 8th, 11th, 19th, and 20th, but not much; the reft of this month was fair.

June. Very wet for 7 or 8 days, and on the 10th, 11th, and 12th, and alfo from the 14th to 17th; it was then fair to the 23d; rained then and on the 24th and 27th.

July. Very wet and cloudy, with thunder, &c. on the 5th and 19th; very few days fair till the 22d; after which it was in general fair this month.

Auguft. Very fine for a few days; but afterwards it became wet and ftormy till the 20th; after which it was in general fair.

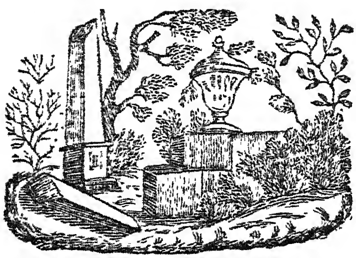
September. Stormy for fome days; fair on the 8th and 9th; frofty on the 10th, and then fair to the 15th; wet to the 18th; and then fair to the end.

October. Dry though cloudy, with a little rain for 7 or 8 days; after which it rained part of every day for as many more, and then became variable with fome frofty nights.

November. Dry with fome froft and fog, but with little rain on the 9th and 11th, and for a fortnight after; it

it next became variable, but generally fair; it rained very hard on the 23d and on the 28th.

December. Wet for a few days and then cloudy, but without rain for the ſirſt week; it was then dry for a few days; it froze on the 16th, and ſnow fell on the 18th; the 19th was fair; it rained afterwards to the 23d, and ſnowed on the 24th; after which a froſt ſat in, and continued till the end.



METEOROLOGICAL JOURNAL

KEPT AT THE HOUSE OF

THE ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL
for January 1776.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Jan. 1	8	0	38,5	39,0	29,94		SE	1	Cloudy.
	2	0	41,0	40,0	29,82		S	1	Fine.
2	8	0	41,0	41,5	29,50	0,075	NNW	1	Rain.
	2	0	40,5	41,5	29,69		NNW	1	Cloudy.
3	8	0	36,0	39,5	30,09	0,065	NE	1	Cloudy.
	2	0	37,0	40,5	30,14		E	1	Fine.
4	8	0	39,0	38,5	29,77	0,031	SSE	1	Rain.
	2	0	44,0	40,0	29,50		S	2	Rain.
5	8	0	44,5	43,5	29,40	0,444	SW	1	Fair.
	2	0	48,5	45,5	29,48		WSW	1	Cloudy.
6	8	0	36,0	43,5	29,63		WSW	1	Fine.
	2	10	41,5	44,0	29,57		E by N	1	Fair.
7	8	30	33,0	39,5	29,245	0,153	ENE	2	Rain.
	2	0	31,0	38,0	29,35		ENE	2	Snow.
8	8	0	30,0	34,0	29,46	0,229	NE	1	Much ſnow laſt night.
	2	0	32,0	35,5	29,49		N	1	Fair.
9	8	0	30,0	34,5	29,63	0,079	N	1	Cloudy.
	2	0	28,5	33,5	29,67		ENE	1	Cloudy.
10	8	0	30,5	33,5	29,73		NNE	1	Cloudy.
	2	0	33,0	34,0	29,73		NW	1	Cloudy.
11	8	0	30,0	31,5	29,57		E	1	Cloudy.
	2	0	35,0	33,5	29,41		SE	1	Cloudy.
12	8	0	30,0	33,0	29,21	0,091	E	2	Much ſnow and wind laſt nt.
	2	0	32,0	34,0	29,21		ENE	2	Snow.
13	8	0	30,0	32,5	29,32		NE	2	Much ſnow and wind laſt nt.
	2	0	29,0	32,0	29,36		NE	2	Cloudy.
14	8	0	25,0	30,0	29,45		ENE	2	Snow.
	2	0	26,0	29,0	29,49		NE	2	Snow.
15	8	0	26,0	29,0	29,70		N	1	Cloudy.
	2	0	29,0	30,5	29,70		ENE	1	Snow.
16	8	0	25,5	29,0	29,71		NNE	1	Snow.
	2	0	30,5	30,5	29,70		NE	1	Snow.

METE-

METEOROLOGICAL JOURNAL.
for January 1776.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H	M.			Inches.	Inch.	Points.	Str.	
Jan. 17.	8	0	28,0	29,5	29,74		NNE	1	Cloudy.
	2	0	30,5	30,5	29,74		NNE	1	Cloudy.
18	8	0	33,0	32,0	29,87		ENE	1	Cloudy.
	2	0	30,0	31,5	29,90		ENE	1	Cloudy.
19	8	0	27,0	30,5	29,90		NE	1	Snow.
	2	0	27,0	31,0	29,85		NE	1	Fair.
20	8	0	23,5	29,0	29,70		N	1	Fair.
	2	0	24,0	29,0	29,70		N	1	Fine.
21	8	0	22,0	27,0	29,62		SSW	1	Cloudy.
	2	0	28,5	27,5	29,55		SSW	1	Fine.
22	8	0	30,5	28,5	29,41		E	1	Fair.
	2	0	33,5	30,5	29,39		NE	1	Fine.
23	8	0	25,0	28,5	29,53		NE	1	Fair.
	2	0	33,0	30,0	29,58		NNE	1	Snow.
24	8	0	30,0	30,0	29,81		ENE	0	Cloudy.
	2	0	35,0	31,5	29,84		NE by E	1	Fair.
25	8	0	29,0	31,5	29,87		NE	1	Cloudy.
	2	0	33,0	32,5	29,85		SE	1	Fair.
26	8	0	26,0	30,5	29,82		E	1	Cloudy.
	2	0	26,0	30,5	29,82		SSE	2	Fine.
27	8	0	19,5	26,0	29,80		ENE	2	Snow.
	2	0	20,5	25,5	29,79		ENE	2	Snow.
28	8	0	18,5	22,5	29,94		E	2	Snow.
	2	0	22,0	23,0	30,00		E	2	Fine.
29	8	0	14,5	20,0	29,91		SE	1	Fine.
	2	0	24,0	21,5	29,88		SE by E	1	Fair.
30	8	0	15,0	19,5	29,97		NE	1	Fair.
	2	0	21,0	20,5	30,02		E	1	Fine.
31	8	0	13,5	19,5	30,09		NE	1	Fair.
	2	0	23,5	20,5	30,07		E	1	Fine.

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for February 1776.

	Time. H.	Time. M.	Therm. without	Therm. within	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
Feb. 1	8	0	14,5	19,0	29,97		NE	1	Fair.
	2	0	31,5	31,5	29,86		E	1	Cloudy.
2	8	0	37,0	28,5	29,78		NE	0	Cloudy.
	2	0	40,5	31,0	29,57		SE by E	1	Fair.
3	8	0	37,0	34,5	29,34	0,093	SSE	1	Fog.
	2	0	44,0	36,0	29,51		S by W	1	Fine.
4	8	20	44,0	38,5	29,22		SW	2	Cloudy.
	2	0	48,0	41,0	29,22		SW	1	Cloudy.
5	8	0	40,5	42,0	29,16	0,153	WSW	2	Fine.
	2	0	48,0	44,0	29,11		SWbyW	2	Fine.
6	8	0	37,0	42,5	28,98	0,221	WSW	2	Fine.
	2	0	45,0	44,0	29,02		WSW	2	Fine.
7	8	0	43,0	42,0	28,91		SWbyW	2	Fair.
	2	0	47,5	43,5	29,07		SW	2	Fair.
8	8	0	40,0	42,5	29,66	0,190	SW	1	Rain.
	2	0	49,5	44,5	29,62		SSW	2	Cloudy.
9	8	0	41,0	44,5	29,78	0,190	SSW	1	Fine.
	2	0	45,5	45,5	29,58		S	2	Cloudy.
10	8	0	42,0	46,0	28,99	0,524	SWbyW	2	Rain.
	2	0	48,0	47,0	28,97		SW	2	Fair.
11	8	0	42,5	45,0	28,84	0,275	SW	2	Fair.
	2	0	45,5	46,0	28,84		SSW	2	Rain.
12	8	0	34,0	42,0	29,10		SSW	1	Fair.
	2	0	46,0	44,0	29,24		WSW	1	Fine.
13	8	0	36,5	42,5	29,68		SW	1	Fine.
	2	0	47,0	44,0	29,71		WSW	1	Fine.
14	8	0	47,0	45,0	29,51	0,163	SW	2	Fair.
	2	0	51,0	48,0	29,53		S by W	1	Fine.
15	8	0	38,0	44,5	29,96		SSW	1	Fine.
	2	0	48,0	46,5	29,91		SSW	2	Fair.
16	8	0	42,0	46,0	29,75	0,085	SW	1	Fine.
	2	0	49,5	47,5	29,78		SW	1	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M			Inches	Inch.	Points.	Str.	
Feb. 17	8	0	36,0	43,0	29,68	0,021	ENE	1	Rainy.
	2	0	43,5	42,5	29,53		E	1	Fair.
18	7	0	35,5	42,0	29,43	0,270	SSW	1	Fair.
	2	0	39,5	42,0	29,25		NE	1	Cloudy.
19	7	0	38,5	42,0	29,34	0,140	SW	1	Rainy.
	2	0	41,5	41,5	29,62		NW	1	Fair.
20	7	0	41,5	40,5	29,775	0,089	S	1	Rainy.
	2	0	50,0	44,0	29,80		W by S	1	Fine.
21	7	0	49,5	45,5	29,58		WSW	2	Cloudy.
	2	0	54,5	49,0	29.50		WSW	2	Fair.
22	7	0	36,5	44,0	29,62	0,412	SW	1	Fine.
	2	0	45,0	45,0	29,41		SSE	3	Rain.
23	7	0	35,0	40,5	29,38	0,148	WSW	1	Fine.
	2	0	45,5	44,0	29,50		WSW	2	Fair.
24	7	0	39,0	41,0	29,25	0,079	SSE	1	Rain.
	2	0	46,0	48,0	29,10		WSW	1	Rain.
25	7	0	37,0	40,0	29,50	0,102	N	1	Cloudy.
	2	0	43,0	41,5	29,55		SSW	1	Fair.
26	7	0	46,0	43,5	29,18		SSW	1	Cloudy.
	2	0	53,0	46,5	29,13		S by W	2	Cloudy.
27	7	0	39,5	43,0	29,155	0,149	SSW	2	Cloudy.
	2	0	48,0	45,0	29,26		WSW	2	Fair.
28	7	0	43,0	43,5	29,11	0,140	SSW	2	Fair.
	2	0	47,0	45,5	29,17		SW	2	Fair.
29	7	0	38,0	43,0	29,35	0,066	SSW	1	Fine.
	2	0	48,0	45,0	29,35		WSW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Mar. 1	7	0	32,0	42,0	29,52	0,080	WSW	1	Fair.
	2	0	45,5	43,5	29,60		NE	1	Fine.
2	7	0	35,0	41,0	29,75		N	1	Fair.
	2	0	43,0	42,5	29,77		NNE	1	Fair.
3	7	0	30,5	37,0	29,78		SSW	1	Fine.
	2	0	44,0	40,5	29,67		SSW	1	Fair.
4	7	0	36,0	38,0	29,49	0,159	N	2	Cloudy.
	2	0	44,5	41,5	29,73		NNW	2	Fair.
5	7	0	44,0	42,5	29,81	0,058	SW	2	Fair.
	2	0	46,0	44,5	29,85		SW	2	Rain.
6	7	0	41,5	44,0	29,77	0,314	SE	1	Rain.
	2	0	43,5	44,5	29,58		E	1	Rain.
7	7	0	39,0	43,0	29,70	0,514	E	1	Cloudy.
	2	0	42,0	43,5	29,59		SSE	1	Rain.
8	7	0	40,0	45,0	29,25	0,229	SSW	1	Fine.
	2	0	49,0	47,0	29,18		SW	2	Fair.
9	7	0	43,0	45,5	28,98	0,065	S	2	Fair.
	2	0	50,0	49,0	28,99		SSW	2	Fair.
10	7	0	40,5	44,5	29,37	0,079	WSW	1	Fair.
	2	0	48,0	46,5	29,50		W	1	Fine.
11	7	0	35,0	41,0	29,72		NNW	1	Fine.
	2	0	46,0	44,0	29,86		NW	1	Fine.
12	7	0	41,5	42,0	29,95		SSW	1	Cloudy.
	2	0	49,0	45,0	29,99		SSW	1	Rain.
13	7	0	46,0	46,5	30,00	0,014	SSW	1	Fair.
	2	0	53,5	49,0	29,86		SSW	1	Fair.
14	7	0	41,5	45,5	29,83		N	1	Fair.
	2	0	49,0	47,0	29,94		N	1	Fine.
15	7	0	40,0	45,5	30,03		W	1	Fine.
	2	30	55,0	48,5	30,05		WSW	1	Cloudy.
16	7	0	43,0	48,0	30,19		N	1	Fine.
	2	0	49,5	49,0	30,21		N	1	Fine.

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	Time.		Threm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch	Points.	Str.	
Mar. 17	7	0	38,0	46,0	30,30		S	1	Fine.
	2	0	53,5	48,5	30,30		WSW	1	Fine.
18	7	0	34,0	45,0	30,18		SW	1	Fine.
	2	0	53,5	48,0	30,13		SW	1	Fine.
19	7	0	47,0	48,0	30,06		SW	1	Cloudy.
	2	0	50,0	46,0	30,09		SW	1	Cloudy.
20	7	0	48,0	49,0	30,15		W	1	Cloudy.
	2	0	55,0	52,0	30,23		SSW	1	Cloudy.
21	7	0	43,5	49,5	30,23		E	1	Fine.
	2	0	59,5	53,5	30,20		SW	1	Fair.
22	7	0	49,0	52,0	29,96		SW	1	Fine.
	2	0	63,5	60,0	29,95		SSW	1	Fine.
23	7	0	46,5	49,5	29,94		NE	1	Fog.
	2	0	66,0	57,0	29,91		SW by S	1	Fine.
24	7	0	52,5	56,0	29,94		NNE	1	Fair.
	2	0	67,0	59,0	29,99		SE	1	Fine.
25	7	0	45,0	52,0	30,30		ENE	1	Fair.
	2	0	49,0	49,0	30,33		ENE	2	Fine.
26	7	0	37,0	42,0	30,38		NE	1	Fair.
	2	0	49,5	46,0	30,33		NE	1	Fine.
27	7	0	33,5	42,0	30,13		NNE	1	Fine.
	2	0	51,5	45,0	30,04		NNE	1	Fine.
28	7	0	41,5	44,5	30,01	0,029	NNE	1	Cloudy.
	2	0	46,0	46,0	30,03		NE	1	Cloudy.
29	7	0	40,0	44,5	30,11		N	1	Fair.
	2	0	47,5	46,0	30,20		NNE	1	Fair.
30	7	0	35,0	44,0	30,37		S	1	Fine.
	2	0	56,5	47,5	30,38		N by W	1	Fine.
31	7	30	44,5	47,5	30,43		WSW	1	Fair.
	2	0	60,5	51,5	30,42		WSW	1	Fine.

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	Time.		Therm without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Apr. 1	7	0	44,0	50,5	30,44		SSW	1	Fine.
	2	0	60,0	54,0	30,43		N	1	Fine.
2	7	0	48,0	53,0	30,37		NNE	1	Cloudy.
	2	0	59,0	54 5	30,34		NE	1	Fine.
3	7	0	42,5	51,5	30,34		SW	1	Fine.
	2	0	62,0	56,5	30,28		W	1	Fine.
4	7	0	45,0	51,0	30,22		N	1	Fine.
	2	0	54,0	53,5	30,22		N	1	Fine.
5	7	0	41,0	48,5	30,22		N	1	Fair.
	2	0	49,0	49,5	30,17		N	1	Fair.
6	7	0	40,5	46,5	30,145		N	1	Cloudy.
	2	0	47,0	48,0	30,11		SSW	1	Cloudy.
7	7	0	45 5	46,0	30,03		WNW	1	Fair.
	2	0	50,5	48,5	29,94		NW	1	Cloudy.
8	7	0	45,0	48,0	29,75		WNW	1	Fair.
	2	0	51,5	50,0	29,57		NW	2	Fine.
9	7	0	39,0	43,5	29,71	0,065	NW	2	Fair.
	2	0	39,0	45,0	29,86		NWbyN	2	Cloudy.
10	7	0	34,5	41,0	30,13	0,052	N	1	Fine.
	2	0	48,5	44,0	30,16		NNE	2	Fine.
11	7	0	44,5	44,0	30,21		NNE	1	Cloudy.
	2	0	54,0	48,0	30,21		N	1	Fair.
12	7	0	44,0	46,0	30,17		NW	1	Fair.
	2	0	53,5	49,5	30,07		WNW	1	Fair.
13	7	0	45,0	48,5	29,85		SW	1	Fair.
	2	0	56,5	52,0	29,80		NW	1	Fair.
14	7	0	48,5	51,0	29,81		SSW	1	Cloudy.
	2	0	57,0	54,5	29,76		WSW	1	Fair.
15	7	0	48,5	52,0	29,68	0,010	SW	1	Cloudy.
	2	0	56,0	53.5	29,68		SW	1	Fair.
16	7	0	48,5	57,5	29.51		E	1	Fair.
	2	0	62,0	55,0	29,46		S	2	Rain.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Apr. 17	7	0	52,0	55,0	29,58	0,132	S	2	Rain.
	2	0	65,5	60,0	29,64		S	1	Fine.
18	7	0	56,5	58,5	29,64	0,061	S	0	Cloudy.
							SW	1	Fair.
19	7	0	44,5	51,0	29,97		SW	1	Fair.
	2	0	60,5	59,0	30,00		SSW	1	Fair.
20	7	0	48,5	53,0	30,04		SW	1	Fair.
	2	0	59,0	57,5	30,07		W	1	Fair.
21	7	0	47,0	50,5	30,27		N	1	Fine.
	2	0	60,0	57,5	30,30		N by W	1	Fine.
22	7	0	48,5	54,0	30,30		S	1	Fair.
	2	0	63,0	58,5	30,25		N	1	Fair.
23	7	0	48,0	53,5	30,16		NE	1	Fair.
	2	0	62,5	59,5	30,09		NE	1	Fine.
24	7	0	48,0	55,0	29,95		NE	1	Fine.
	2	0	64,5	58,5	29,88		ENE	1	Fine.
25	7	0	50,0	57,0	29,86		SE	1	Fair.
	2	0	68,5	62,0	29,88		NW	1	Fine.
26	7	0	43,0	49,5	30,16		N	1	Fine.
	2	0	58,0	55,0	30,19		N	1	Fair.
27	7	0	47,5	51,0	30,30		NW	1	Fine.
	2	0	59,0	55,5	30,30		NE	1	Fine.
28	7	0	46,5	51,5	30,30		SW	1	Fine.
	2	0	63,5	57,5	30,20		WSW	1	Fine.
29	7	0	45,5	52,0	30,10		N	1	Fair.
	2	0	56,5	55,5	30,10		NNW	1	Fair.
30	7	0	41,0	46,0	30,18		NNW	1	Fair.
	2	30	50,5	51,0	30,19		NW	1	Cloudy.

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	Time. H. M.		Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
May 1	7	0	40,0	47,0	30,21		N	1	Cloudy.
	2	0	50,5	49,5	30,21		NNE	1	Cloudy.
2	7	0	49,0	50,0	30,18		W	1	Cloudy.
	3	0	64,5	55,5	30,11		SW	1	Fine.
3	7	0	52,0	55,0	29,70		SW	2	Cloudy.
	2	0	60,5	57,5	29,64		W by S	2	Fine.
4	7	0	46,0	50,0	29,77	0,094	NNW	2	Fine.
	2	0	61,0	55,5	29,65		WSW	1	Fair.
5	7	0	44,5	52,5	29,21	0,105	SW	1	Cloudy.
	2	0	50,5	54,0	29,14		WSW	2	Fine.
6	7	0	42,0	48,5	29,48	0,242	NWbyN	2	Fine.
	2	0	51,5	51,5	29,50		NW	2	Fair.
7	7	0	42,5	48,0	29,76		W by N	1	Fair.
	2	0	50,5	50,5	29,70		N by W	1	Fair.
8	7	0	41,5	48,0	29,66	0,167	N	1	Fair.
	2	0	52,0	51,0	29.67		NNE	2	Fair, Thunder at 1.
9	7	0	45,0	49,0	29,71	0,077	NE	1	Cloudy.
	2	0	52,0	51,5	29,78		NNE	1	Cloudy.
10	7	0	46,0	50,0	30,00	0,010	SE	1	Fine,
	2	0	58,0	53,0	30,05		ENE	1	Fine.
11	7	0	46,5	52,0	30,18	0,071	N	1	Fine.
	2	0	59,0	55,5	30,22		NNE	1	Fair.
12	7	0	53,0	54,5	30,28		SSW	1	Fair.
	2	0	62,0	58,0	30,29		SW	1	Cloudy.
13	7	0	49,0	55,5	30,45	0,067	NE	1	Fine.
	2	0	62,5	57,5	30,46		N by W	1	Fine.
14	7	0	55,0	57,5	30,43		WSW	1	Fair.
	2	0	63,5	61,5	30,36		N by W	1	Fine.
15	7	0	48,0	54,5	30,18		N	1	Rain.
	2	0	55,0	57,0	30,16		N	2	Fair.
16	7	0	46,0	58,0	30,13	0,010	N	1	Fair.
	2	0	54,0	54,5	30,02		N	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M			Inches.	Inch.	Points.	Str.	
May 17	7	0	47,0	52,0	30,03	0,051	N	1	Fair.
	2	0	55,0	55,0	30,06		N by E	1	Fair.
18	7	0	46,0	51,5	30,06		NE by N	1	Rain.
	2	0	56,0	53,0	29,98		NE	2	Fair.
19	7	0	52,0	52,5	29,91	0,110	E by N	2	Fair.
	2	0	50,0	53,5	29,93		NE	2	Rain.
20	7	0	49,5	53,5	30,10	0,590	NNE	1	Rain.
	2	0	64,0	57,0	30,15		NE	1	Fair.
21	7	0	47,5	53,5	30,255		NE	1	Cloudy.
	2	15	58,5	56,0	30,22		NNE	1	Fine.
22	7	0	47,0	53,0	30,15		N	1	Fair.
	2	0	61,5	55,5	30,07		NE	1	Fine.
23	7	0	47,0	52,5	30,08		N	1	Fair.
	2	0	54,0	53,5	30,07		N	1	Fair.
24	7	0	46,5	48,0	30,15		NE	2	Fine.
	2	0	58,5	53,0	30,20		NNW	2	Fine.
25	7	0	44,0	49,5	30,22		S	1	Fine.
	2	0	59,0	53,5	30,16		NNW	1	Fine.
26	7	0	53,0	54,0	30,12		W	1	Fine.
	2	0	65,0	58,5	30,11		WNW	1	Fair.
27	7	0	54,0	57,5	30,09		NW	1	Cloudy.
	2	0	64,5	61,0	30,06		SSE	1	Fair.
28	7	0	57,5	59,0	30,04		SE	1	Fine.
	2	0	72,0	64,0	30,02		SW	1	Fine.
29	7	0	61,5	61,5	29,98		SSE	1	Fine.
	2	0	77,5	67,0	29,97		S	1	Fine.
30	7	0	62,0	66,0	29,87		S	1	Fair.
	2	0	66,0	68,0	29,85		S by E	2	Fair.
31	7	0	60,0	65,0	29,86		S	1	Fair.
	2	0	70,5	68,5	29,87		SSW	1	Fair.

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	Time. H.	M.	Therm. without	Therm. within	Barom. Inches	Rain. Inch.	Winds. Points.	Str.	Weather.
June 1	7	0	58,0	64,5	29,81	0,010	NE	1	Cloudy.
	2	0	60,0	65,5	29,75		E	1	Rainy.
2	7	0	57,5	64,0	29,90	0,215	W	1	Fair.
	2	0	73,0	67,0	29,92		E	1	Fair.
3	7	0	60,0	65.5	29,92	0,070	NNE	1	Cloudy.
	2	0	78,5	70,5	29,92		E	1	Fine.
4	7	0	66,0	70,5	29,83		NNE	1	Fine.
	2	0	74,0	72,5	29,87		SWbyW	1	Fine.
5	7	0	61,0	65,0	29,82		SW	1	Fine.
	2	0	63,5	67,5	29,75		SW	1	Cloudy.
6	7	0	56,0	63,0	29,51	0,189	W	1	Cloudy.
	2	0	59,5	65,0	29,48		NW	1	Fair.
7	7	0	52,5	61,0	29,59	0,138	SW	1	Fine.
	2	0	58,0	61,5	29,63		S by W	1	Rain.
8	7	0	55,0	58,0	29,66	0,249	SW	1	Fine.
	2	0	61,5	60,5	29,68		SW	1	Fine.
9	7	0	55,0	58,5	29.85	0,041	S	1	Rain.
	2	0	63,0	61,5	29,87		S	2	Rain.
10	7	0	59,0	61,0	30,00	0,078	SSW	1	Fair.
	2	0	66,5	63,0	30,00		SSW	1	Fair.
11	7	0	58,0	61,5	29,67	0,307	SE	1	Rain.
	2	0	67,0	65,0	29,64		SSW	1	Fair.
12	7	0	57,0	62,0	29,56	0,016	E	1	Rain.
	2	0	65,5	64,0	29,57		SSE	1	Fair.
13	7	0	57,0	59,0	29,61		W	1	Fine.
	2	0	67,5	63,5	29,64		W	1	Fine.
14	7	0	59,5	63,5	29,81		SW	1	Fine.
	2	0	71,0	65,5	29,87		SW	1	Fair.
15	7	0	62,0	65,0	29,80		S	1	Fine.
	2	0	67,0	67,5	29,89		SW	1	Cloudy.
16	7	0	59,0	65,5	29,73		S	1	Rain.
	2	0	62,0	66,0	29,72		SSW	1	Cloudy.

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	Time. H. M.		Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
June 17	7	0	57,0	64,0	29,82	0,230	W	1	Fine.
	2	0	69,5	66,5	29,99		W	1	Fine.
18	7	0	58,0	64,5	30,14		SW	1	Fine.
	2	0	67,5	70,0	30,21		NW	1	Cloudy.
19	7	0	61,5	66,0	30,32		N	1	Fine.
	2	0	72,0	69,5	30,34		E	1	Fine.
20	7	0	60,0	63,5	30,27		SSW	1	Fine.
	2	0	74,0	68,5	30,19		S	1	Fine.
21	7	0	64,5	65,5	29,94		S	1	Cloudy.
	2	0	72,5	69,5	29,88		SW	1	Cloudy.
22	7	0	60,5	66,5	29,89		WSW	1	Fine.
	2	0	68,5	67,5	29,90		W by S	2	Fair.
23	7	0	58,0	64,5	29,95		W by S	2	Fair.
	2	0	69,5	66,5	29,93		SW	2	Fair.
24	7	0	58,0	54,5	29,76		W	1	Fine.
	2	0	67,0	65,5	29,87		SW	2	Fair.
25	7	0	55,0	62,5	30,03		W	1	Fine.
	2	0	66,0	64,0	30,02		W	1	Fine.
26	7	0	55,0	60,0	29,94		W	1	Fine.
	2	0	68,5	65,0	29,86		NE	1	Cloudy.
27	7	0	56,0	62,0	29,77		NE	1	Cloudy.
	2	0	56,5	63,5	29,79		SE	2	Rainy.
28	7	0	53,5	58,5	29,87	0,465	NNE	1	Cloudy.
	2	0	63,0	67,0	29,94		NE	1	Fair.
29	7	0	53,5	59,0	30,03	0,020	N	0	Cloudy.
	2	0	63,0	63,0	30,05		NW	1	Fair.
30	7	0	57,0	62,0	30,01		N	1	Fair.
	2	0	71,0	65,5	29,99		SWbyW	1	Fair.

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	Time.		Therm without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches	Inch.	Points.	Str.	
July 1	7	0	55,0	62,0	29,95		W by S	1	Fair.
	2	0	69,0	67,0	30,00		W by S	1	Fair.
2	7	0	60,5	65,0	29,98		SW	1	Fair.
	2	0	69,5	68,5	29,98		SW	1	Rain.
3	7	0	5[illegible],0	60,0	29 99	0,101	SW	1	Fair.
	2	0	71,5	70,0	29,97		WSW	1	Fair.
4	7	0	64,0	67,5	29.95		SW	1	Fair.
	2	0	76,0	72.0	29.91		SW	1	Fair.
5	7	0	70,0	70,0	29,77		S	1	Fine.
	2	0	67,0	71,5	29,74		S by W	1	Rain.
6	7	0	63,0	67,0	29,55	0,419	S	1	Cloudy.
	2	0	65,0	69,5	29,59		SW	1	Rain.
7	7	0	59,5	69,5	29,555	0,039	SW	1	Fair.
	2	0	66,5	68,5	29,62		SSW	2	Cloudy.
8	7	0	58,5	64,5	29,94	0,054	SW	1	Fine.
	2	0	70,0	66,5	29 99		SW	1	Fair.
9	7	0	56,0	64,0	29,995		SSW	1	Fair.
	2	0	69,0	66,0	29.96		S by W	1	Fair.
10	7	0	58,5	6[illegible],5	29,91		SW by W	1	Fair.
	2	0	74,0	69,0	29,94		SSW	1	Fair.
11	7	0	56,5	65,5	29,84	0,360	ENE	1	Rain.
	2	0	63,0	68,0	29,85		NE by N	1	Fair.
12	7	0	62,0	66,5	29,855	0,150	SSW	1	Fair.
	2	0	70,5	69,5	29.86		SW	1	Fine.
13	7	0	61,0	67,0	29,77	0,198	W by N	1	Rainy.
	2	0	70,0	70,0	29.76		WSW	1	Fair.
14	7	0	62,0	67,0	29,85		SW	1	Fine.
	2	0	71,5	69,5	29,80		S by W	1	Fine.
15	7	0	62,5	67,5	29,755	0,092	S	1	Fine.
	2	0	65,5	68,5	29,71		S by W	2	Fair.
16	7	0	63,0	67,0	29,58	0,064	S	1	Cloudy.
	2	0	73,0	70,5	29,61		SW	2	Fine.

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	Time.		Threm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
July 17	7	0	64,0	69,0	29,66		SSW	1	Fair.
	2	0	71,0	70,5	29,71		W by S	1	Fair.
18	7	30	62,0	67,5	29,82		SWbyW	1	Fine.
	2	0	74,0	70,0	29,82		SW	2	Fine.
19	7	0	60,5	65,5	29,74		ENE	1	Fair.
	2	0	76,0	71,5	29,68		S	1	Fair.
20	7	0	63,0	68,0	29,71		W by N	1	Fair.
	2	0	72,5	71,0	29,81		W by N	1	Fair.
21	7	0	63,0	67,0	29,87		SSW	1	Cloudy.
	2	0	69,0	69,0	29,85		SSW	1	Fair.
22	7	0	61,0	65,0	29,80	0,275	E by S	0	Cloudy.
	2	0	66,0	67,0	29,84		NE	1	Cloudy.
23	7	0	59,0	65,0	29,90		SW	1	Fine.
	2	0	74,0	79,5	29,95		SWbyW	1	Fine.
24	7	0	63,0	68,5	30,03		SW	1	Fair.
	2	0	67,0	69,5	30,05		S	1	Rainy.
25	7	0	57,0	63,5	30,22		SSW	1	Fine.
	2	0	69,0	67,5	30,23		SW	1	Fine.
26	7	0	61,0	67,0	30,13		SW	1	Fine.
	2	0	82,0	73,0	30,08		S by W	1	Fair.
27	7	0	62,5	67,5	30,15		SW	1	Fine.
	2	0	73,0	70,5	30,18		W	1	Fair.
28	7	0	59,5	67,0	30,22		SW	1	Fine.
	2	0	71,5	70,5	30,17		SSW	1	Fair.
29	7	0	62,0	66,0	30,18		NW	1	Fine.
	2	0	72,0	70,5	30,22		W by N	1	Fine.
30	7	0	61,0	65,0	30,31		NE	1	Fair.
	2	0	74,0	70,5	30,29		E by N	1	Fine.
31	7	0	62,0	65,0	30,24		SE	1	Fine.
	2	0	75,0	72,0	30,21		E by S	1	Fine.

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	Time. H.	M.	Therm. without	Therm. within.	Barom Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Aug. 1	7	0	65,0	66,0	30,16		SW	1	Fair.
	2	0	74,0	67,0	30,14		S by W	1	Fine.
2	7	0	67,0	68,5	30,13		E	1	Fair.
	2	0	86,0	77,0	30,09		S	1	Fine.
3	7	0	62,0	72,5	30,09	0,052	W by S	1	Fine.
	2	0	76,0	75,0	30,06		N	1	Fine.
4	7	0	63,5	68,5	30,03		W by N	1	Fair.
	2	0	73,0	73,0	30,00		NNW	1	Fair.
5	7	0	66,0	70,5	29,92		SW	1	Fine.
	2	0	79,5	74,5	29,85		WSW	1	Fine.
6	7	0	63,5	71,5	29,77		NW	1	Fair.
	2	0	72,0	72,5	29,73		W by N	1	Fair.
7	7	0	56,0	64,0	29,82		SW	1	Fine.
	2	0	67,5	67,0	29,84		W	1	Fair.
8	7	0	55,0	61,0	29,64	0,262	SE	1	Rainy.
	2	0	65,0	64,5	29,76		NW	1	Fine.
9	7	0	59,5	63,5	29,80	0,067	SE by S	1	Fine.
	2	0	66,5	66,0	29,71		SSE	2	Cloudy.
10	7	0	57,0	63,5	29,53	0,452	SSW	1	Rainy.
	2	0	66,0	65,5	29,65		W	2	Fair.
11	7	0	67,0	65,0	29,76	0,129	SE	1	Fair.
	2	0	66,5	66,5	29,71		S	1	Rain.
12	7	0	60,0	64,0	29,75	0,306	SW	1	Fine.
	2	0	66,5	66,0	29,73		SSW	1	Rain.
13	7	0	59,0	64,0	29,70	0,169	SW by S	1	Fine.
	2	0	67,5	68,0	29,70		W	2	Fine.
14	7	0	55,5	63,5	29,96	0,054	SW	1	Fine.
	2	0	71,5	68,0	29,98		SW	1	Fine.
15	7	0	64,5	67,0	29,88	0,022	SSW	1	Cloudy.
	2	0	73,5	71,5	29,80		SW by S	1	Fine.
16	7	0	63,0	67,5	29,61	0,161	S by W	1	Rain.
	2	0	69,5	70,0	29,63		SSW	2	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Aug. 17	7	0	55,0	63,0	29,76	0,201	WSW	1	Fine.
	2	0	67,0	66,0	29,81		WSW	2	Fair.
18	7	0	54,0	62,0	29,875	0,018	W	1	Fine.
	2	0	69,5	65,5	29,88		W	1	Fine.
19	7	0	61,5	64,0	29,64	0,139	S	1	Rain.
	2	0	69,5	68,0	29,64		SW	1	Fair.
20	7	0	59,0	65,5	29,69	0,250	W by S	2	Fair.
	2	0	67,5	67,0	29,82		W by S	2	Fine.
21	7	0	57,5	63,0	30,08		ENE	1	Fair.
	2	0	67,0	67,0	30,09		NE	1	Fine.
22	7	0	54,5	58,0	30,07		NNW	1	Fine.
	2	0	67,0	65,5	30,06		NW	1	Fine.
23	7	0	57,0	62,5	30,06		NE	1	Fair.
	2	0	70,0	66,5	30,05		NE	1	Fine.
24	7	0	54,0	62,0	30,06		W by N	1	Fine.
	2	0	64,5	64,5	30,07		NW	1	Fair.
25	7	0	53,0	61,0	30,10		W	1	Fine.
	2	0	66,0	64 0	30,13		NW	1	Fine.
26	7	0	57,5	61,5	30,07		E	1	Cloudy.
	2	0	67,0	66,0	30,03		S	1	Cloudy.
27	7	0	57,0	62,5	29,93	0,050	E	1	Cloudy.
	2	0	64,5	66,5	29,93		E	1	Fair.
28	7	0	53,0	60,0	29,85		NE	1	Fine.
	2	0	69,5	65,5	29,84		SE	1	Fair.
29	7	0	59,0	62,0	29,55	0,050	S by E	1	Rainy.
	2	0	69,0	67,5	29,99		SW	1	Fair.
30	7	0	57,0	64,5	29,60	0,136	S	1	Fine.
	2	0	66,0	67,5	29,60		SW	1	Fair.
31	7	0	54,0	63,5	29,69	0,022	SSW	1	Fine.
	2	0	70,0	68,5	29,66		SSW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Sept. 1	7	0	55,5	54,0	29,48	0,120	SW	1	Fine.
	2	0	68,5	68,5	29,52		SSW	2	Fine.
2	7	0	58,5	64,5	29,30	0,248	S	2	Fine.
	2	0	63,5	67,5	29,26		SW	2	Fine.
3	7	0	56,0	62,5	29,29	0,358	SW	1	Rainy.
	2	0	63,0	66,0	29,41		SSW	2	Cloudy.
4	7	0	55,0	62,5	29,41	0,084	SW	1	Fair.
	2	0	63,0	65,5	29,49		W by N	1	Fine.
5	7	0	51,0	61,0	29,65	0,054	SW	1	Fair.
	2	0	63,0	65,0	29,72		NW	1	Fair.
6	7	0	53,0	60,5	29,92	0,045	SSW	1	Fair.
	2	0	65,5	65,0	30,00		W by S	1	Fair.
7	7	0	52,5	60,0	29,84		S	1	Fine.
	2	0	66,5	65,5	29,75		S by E	1	Fair.
8	7	0	50,0	61,0	29,48	0,160	SW	1	Fair.
	2	0	57,5	63,5	29,48		NW	1	Fine.
9	7	0	48,0	57,0	29,90	0,058	N by W	1	Fine.
	2	0	66,5	63,5	29,97		NE	1	Fine.
10	7	0	53,0	60,0	30,11		N	1	Cloudy.
	2	0	63,0	63,5	30,12		SSW	1	Fair.
11	7	0	57,0	61,5	30,10		SW	1	Fine.
	2	0	68,5	66,5	30,11		SW	1	Fair.
12	7	0	60,0	64,0	30,07		SSW	1	Cloudy.
	2	0	68,5	68,0	30,08		S	1	Fair.
13	7	0	57,0	64,5	30,07		S	1	Fair.
	2	0	67,0	67,0	30,00		S by W	1	Fine.
14	7	0	57,0	64,0	29,90		NW	1	Fair.
	2	0	64,0	68,0	29,92		NW	1	Fair.
15	7	0	5[illegible],5	62,5	29,99		NE	1	Cloudy.
	2	0	61,0	65,0	29,88		NE	1	Cloudy.
16	7	0	61,5	53,0	29,91	0,377	NW	1	Cloudy.
	2	0	61,5	64,5	29,98		W by N	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Sept. 17	7	0	55,0	62,0	29,99		WSW	1	Cloudy.
	2	0	54,5	63,0	29,84		E by N	1	Rain.
18	7	0	49,0	57,5	29,86	0,638	N	1	Cloudy.
	2	0	57,0	61,0	29,94		N	2	Fair.
19	7	0	47,0	54,0	30,05	0,082	N	1	Fair.
	2	0	50,0	57,5	30,12		NE	1	Rain.
20	7	0	44,5	52,5	30,25	0,178	NNE	1	Fine.
	2	0	52,0	57,5	30,26		NNE	1	Fine.
21	7	0	43,0	52,0	30,30	0,160	N	1	Fine.
	2	0	57,5	57,0	30,35		E	1	Fair.
22	7	0	46,5	52,5	30,33		NNE	1	Fair.
	2	0	59,0	57,5	30,23		E	1	Fine.
23	7	0	45,5	53,5	29,99		NE	1	Fine.
	2	0	60,0	58,0	29,92		NE	1	Fair.
24	7	0	48,0	55,0	29,91		N	1	Fair.
	2	0	63,5	60,5	29,71		NE	1	Fine.
25	7	0	54,0	57,5	29,70		SE	1	Cloudy.
	2	0	66,0	62,5	29,57		SE	1	Fine.
26	7	0	56,5	61,5	29,30	0,131	S	1	Fine.
	2	0	63,0	65,0	29,30		S	2	Fair.
27	7	0	48,0	58,5	29,55		S	1	Fair.
	2	0	61,0	62,5	29,64		S by W	1	Fine.
28	7	0	57,0	60,0	29,52	0,019	SSW	1	Rain.
	2	0	62,0	63,0	29,59		SW	1	Fine.
29	7	0	49,5	59,0	29,80		WNW	1	Fine.
	2	0	61,5	62,5	29,97		NE	1	Fine.
30	7	0	51,0	58,0	29,99		NNE	1	Fair.
	2	0	61,5	62,5	30,00		NE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Oct. 1	7	0	52,0	58,5	29,92		NE	1	Fair.
	2	0	59,5	62,0	29,92		ENE	1	Fair.
2	7	0	54,5	60,0	30,05		NE	1	Cloudy.
	2	0	66,5	64,5	30,10		NNE	1	Fair.
3	7	0	55,0	62,0	30,21		NE	1	Cloudy.
	2	0	60,0	64,0	30,21		N	1	Cloudy.
4	7	0	53,0	65,0	30,10		SSE	1	Cloudy.
	2	0	58,0	63,0	29,98		NW	1	Fair.
5	7	0	46,5	57,5	29,83		WNW	1	Fair.
	2	0	56,5	60,5	29,80		NNW	1	Fair.
6	7	0	44,5	55,0	29,78		NNE	1	Fair.
	2	0	55,0	57,5	29,77		NNW	1	Fine.
7	7	0	42,0	52,0	29,76		SSW	1	Fine.
	2	0	58,5	57,5	29,65		SW	2	Rain.
8	7	0	49,0	55,0	29,84	0,020	S	1	Fair.
	2	0	61,0	59,5	29,75		S	2	Cloudy.
9	7	0	43,5	54,5	30,17	0,012	S	1	Fine.
	2	0	58,0	58,5	30,23		WSW	2	Fine.
10	7	0	57,0	58,5	30,12		WSW	1	Fair.
	2	0	66,5	63,5	30,18		W	1	Fair.
11	7	0	53,0	60,0	30,17		WSW	1	Fine.
	2	0	62,5	64,5	30,19		NNW	1	Fair.
12	7	0	49,5	58,5	30,28		N	1	Fair.
	2	0	57,5	61,5	30,32		NE	1	Fine.
13	7	0	52,0	58,0	30,28		SW	1	Cloudy.
	2	0	58,5	62,0	30,24		NW	1	Cloudy.
14	7	0	51,0	59,0	30,17		W	1	Cloudy.
	2	0	57,0	61,5	30,16		S	1	Cloudy.
15	7	0	57,5	58,5	30,16		NNW	1	Cloudy.
	2	0	57,0	61,5	30,10		SE	1	Cloudy.
16	7	0	44,0	53,5	29,92		NE	1	Foggy.
	2	0	61,0	60,0	29,88		S by E	1	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H	M			Inches.	Inch.	Points.	Str.	
Oct. 17	7	0	58,0	60,0	29,80		S	1	Fair.
	2	0	58,0	62,5	29,85		NNW	1	Rain.
18	7	0	43,5	55,0	30,23	0,152	SW	1	Fair.
	2	0	59,0	60,0	30,19		S	1	Fine.
19	7	0	52,5	57,0	29,86		E by N	1	Foggy.
	2	0	61,5	61,5	29,80		S	1	Rain.
20	7	0	56,0	59,0	29,73		S	1	Fair.
	2	0	56,0	62,0	29,84		W by N	1	Rain.
21	7	0	46,0	55,5	30,01	0,356	SSE	1	Cloudy.
	2	0	59,0	60,0	29,97		S by E	1	Fine.
22	7	0	55,5	58,0	29,99		S	1	Fair.
	2	0	65,0	64,0	30,04		S	1	Rain.
23	7	0	55,0	60,0	30,10		NE	1	Cloudy.
	2	0	56,0	62,0	30,12		NE	1	Cloudy.
24	7	0	56,0	60,0	30,14		E by N	1	Cloudy.
	2	0	55,0	61,5	30,14		NE	1	Cloudy.
25	7	0	51,0	56,5	30,01		NE	1	Cloudy.
	2	0	54,0	59,0	29,93		NE by E	1	Fair.
26	7	0	48,0	54,0	29,84		N	1	Cloudy.
	2	0	51,0	56,5	29,85		NNE	1	Fair.
27	7	0	43,5	51,5	29,90		NNW	1	Cloudy.
	2	0	52,0	55,5	29,93		N	1	Fine.
28	7	0	38,0	49,5	30,04		NE	1	Fair.
	2	0	51,0	55,0	30,04		E	1	Fine.
29	7	0	49,0	52,5	30,03		SE	1	Cloudy.
	2	0	53,0	55,5	30,00		ESE	1	Rain.
30	7	0	49,0	54,0	30,01	0,194	SE	1	Fair.
	2	0	54,0	57,5	30,96		SSE	1	Fine.
31	7	0	43,0	51,0	29,85		ENE	1	Cloudy.
	2	0	48,0	53,5	29,89		NE	1	Fine.

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	Time. H.	Time. M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
Nov. 1	7	0	37,5	47,5	29,94		E	1	Fair.
	2	0	50,0	52,0	29,82		S by E	1	Fair.
2	7	0	39,0	48,5	29,78	0,165	SSW	1	Fair.
	2	0	49,5	52,0	29,78		W by N	1	Fair.
3	7	0	33,5	45,0	30,06		S	1	Fair.
	2	0	44,0	49,0	30,11		SW	1	Fair.
4	7	0	38,5	44,5	30,16		SW	1	Fine.
	2	0	54,0	50,0	30,14		S	1	Fine.
5	8	0	49,0	50,0	30,18		SSW	1	Fair.
	2	0	54,0	54,0	30,19		NW	1	Cloudy.
6	8	0	36,0	47,5	30,17		NE	1	Foggy.
	2	0	44,0	50,5	30,10		NE	1	Fair.
7	8	0	40,0	46,5	30,02		NE	1	Foggy.
	2	0	51,5	51,0	30,00		NE	1	Foggy.
8	8	0	40,0	49,0	30,01		NE	1	Foggy.
	2	0	54,0	56,0	29,98		NE	1	Foggy.
9	8	0	40,5	47,0	29,92		N by E	1	Foggy.
	2	0	55,0	51,5	29,92		S by E	1	Fair.
10	8	0	49,0	51,5	29,97		SE	1	Foggy.
	2	30	52,5	54,5	29,97		S	1	Cloudy.
11	8	0	49,0	53,5	29,90		S	1	Fair.
	2	0	50,5	56,0	29,85		SE	1	Cloudy.
12	8	0	42,0	52,5	30,02	0,136	SW	1	Fair.
	2	0	52,0	55,5	30,11		W by S	1	Fine.
13	8	0	48,0	53,0	30,13		SW	1	Fine.
	2	0	55,5	56,5	30,13		SW	1	Fine.
14	8	0	52,5	55,5	30,23	0,010	SW	1	Cloudy.
	2	0	57,0	58,5	30,21		SW	1	Cloudy.
15	8	0	48,5	55,5	30,22	0,025	SW	1	Fair.
	2	0	56,0	57,5	30,12		SW	1	Fair.
16	8	0	57,5	58,0	29,73		SW	1	Cloudy.
	2	0	55,0	59,5	29,95		SW	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches	Inch.	Points.	Str.	
Nov. 17	8	0	45,5	55,0	29 67	0,010	SW	1	Fair.
	2	0	49,5	56,0	29,77		W	1	Fair.
18	8	0	42,0	50,0	29,87		W	1	Fair.
	2	0	51,5	53,0	29,74		SW	1	Fair.
19	8	0	49,0	53,0	29,71		S	0	Rain.
	2	0	52,0	55,0	29,67	0,140	SE	1	Rain.
20	8	0	53,0	56,0	29,32		SW	2	Fair.
	2	0	58,0	59,5	29,36	0,089	SW	2	Rain.
21	8	0	40,0	49,5	29,70		WNW	2	Fine.
	2	0	45,0	51,0	29,75	0,080	SW	1	Fine.
22	8	0	33,0	45,0	29,94		SW	1	Fair.
	2	0	43,0	48,0	29,89		W	1	Fair.
23	8	0	40,0	45,5	29,36		S by E	1	Rain.
	2	0	43,5	48,0	29,23	0,840	SE	1	Rain.
24	8	0	36,0	44,0	29,61		N by E	2	Fair.
	2	0	42,0	46,5	29,94	0,176	NW	2	Fine.
25	8	0	33,0	41,0	30,16		N	1	Fine.
	2	0	41,0	45,0	30,20		N by E	1	Fine.
26	8	0	28,0	39,5	30,39		E by N	1	Foggy.
	2	0	37,0	43,5	30,31		NE	1	Fine.
27	8	0	31,0	37,0	30,09		E	1	Frosty.
	2	0	36,0	40,0	29,97		SE	1	Fair.
28	8	0	34,0	48,5	29,655		SE	1	Cloudy.
	2	0	35,0	42,0	29,52		SE	1	Cloudy.
29	8	0	38,0	40,5	29,11		SE	1	Rain.
	2	0	46,5	45,0	29,11		SE	1	Cloudy.
30	8	0	41,0	45,5	29,37	0,520	SW	1	Rain.
	2	0	47,0	40,0	29,44		SW	1	Fine.

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	Time.		Therm without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Dec. 1	8	0	50,0	50,0	29,48	0,222	S	2	Fair.
	2	0	54,0	54,0	29,69		SSW	2	Cloudy.
2	8	0	47,0	51,5	30,04		SE	1	Rain.
	2	0	52,5	54,5	29,97		E	1	Fair.
3	8	0	44,5	51,5	29,985		E	1	Foggy.
	2	0	51,0	54,0	29,96		SE	1	Fair.
4	8	0	50 5	53,5	29,95		SSW	1	Foggy.
	2	0	55,0	56,0	29,97		S by W	1	Cloudy.
5	8	0	49,5	50,0	30,01		SE	1	Cloudy.
	2	0	53,0	56,0	30,06		N by E	1	Cloudy.
6	8	0	47,0	54,5	29,95	0,052	NE	1	Cloudy.
	2	0	48,0	55,0	29,91		NE	1	Cloudy.
7	8	0	42,0	50,0	29,95		ENE	1	Fair.
	2	0	43,5	52,0	29,97		S by E	1	Fine.
8	8	0	43,0	47,0	30,05		E	1	Fair.
	2	0	50,5	52,0	30,06		SE	1	Fair.
9	8	0	42,0	49,5	30.10		SSE	1	Fine.
	2	0	44,0	49,5	30,11		SE	1	Fine.
10	8	0	42,0	47,0	30,20		S	1	Cloudy.
	2	0	46,0	48,5	30,25		SW	1	Cloudy.
11	8	0	46,0	48,5	30,35		SW	1	Fine.
	2	0	53,0	51,5	30,41		SW	1	Fine.
12	8	0	49,0	52,0	30,42		SW	1	Cloudy.
	2	0	57,5	52,5	30,39		W	1	Cloudy.
13	8	0	47,0	57,5	30,34	0,057	NE	1	Cloudy.
	2	0	47,0	52,0	30,32		NE	1	Rain.
14	8	0	46,0	50,5	30,33	0,055	SSW	1	Cloudy.
	2	0	47,0	51,0	30,27		SE	1	Cloudy.
15	8	0	39,0	47,0	30,09		SE	1	Fair.
	2	0	42,0	47,0	29,99		SW	1	Cloudy.
16	8	0	40,0	46,0	29,73	0,409	N	1	Rain.
	2	0	44,0	46,0	29,73		NW	1	Fine.

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	Time. H.	M.	Threm. without	Therm: within.	Barom. Inches	Rain. Inch	Winds Points.	Str	Weather.
Dec. 17	8	0	30,0	41,0	29,755	0,020	NNW	1	Fine.
	2	0	36,0	41,0	29,755		NW	1	Fine.
18	8	0	26,0	35,0	29,59		WNW	1	Cloudy.
	2	0	32,0	35,0	29,55		S by W	1	Frosty.
19	8	0	38,0	36,5	29,16	0,139	W by N	2	Rain.
	2	0	40,0	3[illegible],0	29,32		W	1	Fair.
20	8	0	35,0	37,0	29,47		SSW	1	Cloudy.
	2	0	36,0	39,0	29,47		NE	1	Rain.
21	8	0	52,0	39,0	29,69	0,109	SW	1	Rain.
	2	0	54,0	41,0	29,58		SW	1	Rain.
22	8	0	52,0	46,0	29,765	0,053	WSW	2	Cloudy.
	2	0	54,0	46,0	29,84		WSW	1	Fair.
23	8	0	42,0	48,0	29,87	0,050	SW	1	Fair.
	2	0	46,5	48,5	29,81		WNW	1	Fine.
24	8	0	39,0	44,0	29,57	0,022	NW	2	Fair.
	2	0	39,0	44,0	29.51		W by N	2	Rain.
25	8	0	35,5	38,5	29,77	0,047	NW	2	Fine.
	2	0	39,0	40.0	29,82		N by W	1	Fine.
26	8	0	30,0	3[illegible],5	29,81		N	1	Fine.
	2	0	36,0	37,0	29,76		NE	1	Rain.
27	8	0	33,0	36,0	29.98		N	1	Cloudy.
	2	0	36,0	37,0	29 87		NE	1	Cloudy.
28	8	0	29,0	35,0	30,00		N	1	Fair.
	2	0	33,0	35,0	30,02		NNE	1	Fair.
29	8	0	32,0	3[illegible],5	29 73		N	1	Fair.
	2	0	36,5	34,5	29,64		W by N	1	Fair.
30	8	0	32,0	34,5	29,47	0,040	N	1	Snowy.
	2	0	29,0	34,0	29,49		N by E	1	Fine and frosty.
31	8	0	25,0	30,5	29,71		N	1	Fair and frosty.
	2	0	31,0	31,0	29,88		NW	1	Fair and frosty.

1776.	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greateſt Height.	Leaſt Height.	Mean Height.	Greateſt Height.	Leaſt Height.	Mean Height.	Greateſt Height.	Leaſt Height.	Mean Height.	Inches.
January	44,5	13,5	29,3	43,5	20,5	31,8	30,14	29,21	29,687	1,157
February	49,5	14,5	42,6	46,0	19,0	42,4	29,97	28,84	29,408	3,510
March	52,0	30,5	45,9	56,0	37,0	46,4	30,43	28,98	29,91	1,541
April	56,5	34,5	50,3	58,0	41,0	51,2	30,44	29,46	29,542	0,320
May	62,0	40,0	54,3	66,0	47,0	54,9	30,46	29,14	29,999	1,594
June	66,0	32,5	62,4	70,5	54,5	64,2	30,34	29,48	29,87	2,028
July	70,0	55,0	65,9	70,0	60,0	68,3	30,31	29,55	29,913	1,752
Auguſt	67,0	53,0	64,2	72,5	58,0	66,1	30,16	29,53	29,546	2,540
September	61,5	43,0	57,2	64,5	52,0	66,1	30,35	29,26	29,83	2,712
October	58,0	42,0	53,8	65,0	51,0	53,4	30,32	29,65	30,00	0,734
November	57,0	28,0	45,0	58,0	37,0	49,9	30,39	29,11	29,878	1,191
December	52,0	25,0	42,0	57,5	30,5	45,0	30,42	29,16	29,882	1,275
Whole Year,			51,1			52,9			29,789	20,354

VARIATION-NEEDLE.

	7 h. A.M.	12 h. M	2 h. P.M.	10 or 11 h. P.M.	Daily Mean.
June 21			21 52	21 45	21 49
22	21 38	21 49	21 52	22 20	21 55
23	21 43	21 42	21 43	21 47	21 44
24	21 41	21 49	21 54	21 36	21 45
25	21 57	21 54	21 53	21 41	21 51
26	21 43	21 47	21 49	21 33	21 43
27	21 38	21 48	21 50	21 37	21 43
28	21 43	21 47	21 40	21 37	21 42
29	21 59	21 51	21 45	21 56	21 48
30	21 39	21 49	21 44	21 34	21 42
July 1	22 00	21 51	21 50	21 40	21 50
2	21 36	21 50	21 54	21 46	21 47
3	21 59	21 49	21 50	21 46	21 51
4	21 40	21 55	21 52	21 47	21 49
5	21 40	21 56	21 56	21 46	21 49
6	21 40	21 50	21 53	21 35	21 49
7	22 00	21 49	21 49	21 53	21 53
Means,	21 46	21 50	21 50	21 43	
Mean of all 21 47.					

DIPPING-NEEDLE.

	7 h. A.M.	12 h. M.	2 h. P.M.	10 or 11 h. P.M.	Mean.	
	° ′	° ′	° ′	° ′	° ′	
June 21			71 32	71 36		
22	72 22	71 40	71 40	71 45	71 43	Weſt mark uppermoſt.
23	71 45	71 50	71 45	71 30		
24	71 50	71 35	71 45	71 35		
25	71 37	73 10	73 00	72 45		
26	73 20	72 50	73 05	73 00	73 01	Eaſt.
27	73 05	73 00	73 15	72 20		
28	73 10	71 50	71 50	71 50		
29	72 05	72 20	72 00	71 40		
30	71 35	71 50	71 50	72 10	71 56	Eaſt mark down.
July 1	72 00	71 50	71 50	71 50		
2	71 55	70 50	71 52	71 50		
3	71 50	71 50	71 55	72 00		
4	72 00	71 57	73 10	73 30		
5	73 20	73 25	73 20	73 30	73 22	Weſt.
6	73 30	73 24	73 20	73 20		
7	73 25	73 15	73 20	73 25		
					72 30	

THE END OF PART I. OF VOL. LXVII.

ERRATA IN VOL. LXVI.

Page.	Line.
140.	3. from the bottom, *for* Grifons *read* Grifones
144.	2. from the bottom, *for* Elinot *read* Klinot
613.	20. *for* viz. green, *read* viz. white and pearly, green, &c.
618.	11. *for* thefe *read* the
Ibid.	15. *for* fine collection *read* few collections
620.	17. *for* Poulafent *read* Poulaoent
Ibid.	18. *for* Tfchoppan *read* Tfchoppau
Ibid.	20. *for* Freibourg *read* Friebourg
621.	10. *for* powder *read* powdered

*** There are SIX PLATES in the FIRST PART of the PHILOSOPHICAL TRANSACTIONS, Vol. LXVII. But the SECOND of them, containing Two diſtinct Subjects, is numbered TAB. II. at the Head, and TAB. III. below.—This is mentioned, leſt the Binder, attending to the Number at the Head only, ſhould ſuppoſe the THIRD Plate to be wanting.

PHILOSOPHICAL

TRANSACTIONS.

PART II.

PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXVII. For the Year 1777.

PART II.

LONDON,

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MDCCLXXVIII.

CONTENTS

TO

VOL. LXVII. PART II.

XXXI. *A*

PHILOSOPHICAL TRANSACTIONS.

XX. *An Account of a Volcanic Hill near* Invernefs. *In a Letter from* Thomas Weft, *Efq. to Mr.* Lane, *F. R. S.*

DEAR SIR, Bath, Oct. 27, 1776.

Read Dec. 12, 1776.

AS you feem defirous to have a more particular account of the lava, which I left at your houfe on my return from my tour in Scotland, I herewith fend it you. At the fame time I muft own, that the little attention paid to fo extraordinary a phenomenon, and which feems to prove beyond a doubt the exiftence of volcanos in this country, is to me matter of great furprize.

The hill from whence I took the fpecimens of lava which I have the pleafure to fend you, is about a mile

and an half diftant from Invernefs, and is called by the inhabitants Creck Faterick, or Peter's Rock: the lower part of it is chiefly ploughed up; the upper part is extremely fteep, craggy, and very difficult of accefs, and appears to me to have evident marks of having been a volcano, as by much the greateft part of the largeft rocks on it have been either ftrongly calcined or fufed, as will, I think, plainly appear by thefe fpecimens, fome of which I picked up on the fides of the hill, others I broke off from the folid rock with a pick-axe (though not without great difficulty, on account of the extreme hardnefs of the rock); and others I dug out from the fummit of the hill, at the depth of four feet, lying in a rich, light, black mould, which, after having been expofed to the air for fome time, turned to a whitifh afh colour.

On the fummit of this hill (which commands a moft beautiful and extenfive profpect) is a fmall plane, ninety paces long, by twenty-feven wide, furrounded by rocks, from fix to eight feet high, like a breaft-work, extremely craggy, and difficult of accefs on the outfide, but rifing from the center, in the infide, to the top of the breaft-work, with a gentle flope of turf, equal to any of the fineft on our fheep downs. This I at firft thought might have been the crater; but the fmoothnefs of the infide feeming not to favour this opinion, I carefully examined

every part of the hill, in hopes of finding fome marks of one, but without fuccefs. There are, indeed, feveral fmall caves, but of no great depth, which feem to be formed by the hanging rocks on the fides, near the top of the hill. There is alfo a fmall fpring about fifty yards from the top of the hill, but it was almoft dry when I faw it, which was the latter end of July. I could not get, from the beft inquiries I was able to make, any account of this extraordinay phenomenon, no perfon in Invernefs (except one gentleman of that city) having ever taken the leaft notice of it; nor can I find that any mention has ever been made of it in hiftory, though it feems probable that there has been at leaft one more in this part of the country; as a gentleman near Dingwal, which is about twenty miles from Invernefs, affured me, that there were many ftones on a hill near his houfe which they called the vitrified fortification, that had the fame appearance with thofe at Creck Faterick; but that no perfon had ever taken any particular notice of them[(a)].

I am, &c.

(a) The fpecimens fent with the above letter to the Royal Society, having been examined by fome members well acquainted with volcanic productions, were by them judged to be real lava; and it was their opinion, that if a great quantity of the like fubftance be found on the hill from whence thefe pieces were taken, the hill moft probably owes its origin to a volcano. J. P.

XXI. *New Electrical Experiments and Obſervations; with an Improvement of Mr.* Canton's *Electrometer. By Mr.* Tiberius Cavallo, *in a Letter to Mr.* Henly, *F. R. S.*

Iſlington,
February 8, 1777.

DEAR SIR,

Read March 13, 1777.

TOGETHER with this letter I ſend you an account of ſome electrical experiments I have lately made, and moſt of which have been ſucceſsfully repeated in your preſence. As you did me the honour to mention, in your laſt paper to the Royal Society, ſome remarks I had made on Mr. VOLTA's machine, I hope you will farther oblige me by preſenting this account to that learned Body, if you think it contains any thing deſerving the attention of the curious.

I am, &c.

Experiments

Experiments on Mr. VOLTA's *plates, commonly called a machine for exhibiting perpetual electricity.*

THE following experiments, which ſhew how both ſides of an electric plate are affected in different circumſtances, were principally made with a plate which meaſured ſix inches and a quarter in diameter, and which conſiſted of a circular piece of thick glaſs coated on one ſide with ſealing-wax[a].

If, after having excited the ſealing-wax, I lay the plate with the wax upon the table, and the glaſs uppermoſt, that is, contrary to the common method; then, on making the uſual experiment of putting the metal plate on it, and taking the ſpark, &c. I obſerve it to be attended with the contrary electricity; that is, if I lay the metal plate upon the electric one, and, while in that ſituation, touch it with an inſulated body, that body acquires the

(a) Having conſtructed ſeveral of thoſe plates, with a view to diſcover which ſubſtance would anſwer the beſt for coating the glaſs plate, I obſerved that the eaſieſt to be made, and the ſtrongeſt in power, are thoſe made of the ſecond ſort of ſealing-wax. It is remarkable, that ſometimes they will not act well at firſt, but they may be rendered very good by ſcraping with the edge of a knife their ſhining or gloſſy ſurface. This ſeems analogous to the well-known property of glaſs, which is, that new cylinders or globes made for electrical purpoſes are often very bad electrics at firſt, but that they improve by being worked, that is, by having their ſurface a little worn.

pofitive electricity, and the metallic removed from the electric plate appears to be negative.

This experiment, I find, anfwers in the fame manner if an electric plate be ufed which has the fealing-wax coating on both fides; for, whichever fide of this is excited, it will act like the waxed fide of the above defcribed plate, that is, will render the metal plate fet on it pofitive, and the oppofite fide will render it negative.

If, inftead of laying the electric plate upon the table, it be placed upon an electric ftand fo as to be accurately infulated, then the metal plate fet on it acquires fo little electricity that it can only be difcovered with an electrometer; which fhews, that the electricity of this plate will not be confpicuous on one fide of it, if the oppofite fide be not at liberty either to part with, or acquire more of, the electric fluid. In confequence of this experiment, and in order to afcertain how the oppofite fides of the electric plate would be affected in different circumftances, I made the following experiments.

Upon an electric ftand I placed a circular tin plate, nearly fix inches in diameter, which by a flender wire communicated with an electrometer of pith-balls, which was alfo infulated; I then placed the excited electric plate of fix inches and a quarter diameter upon the tin plate, with the wax uppermoft, and on removing my hand from

from it, the electrometer which communicated with the tin plate, that is, with the under side of the electric plate, immediately opened with negative electricity. If by touching the electrometer I take that electricity off, the electrometer will not afterwards diverge. But if now, or when the electrometer diverges, I present my hand open, or any other uninsulated conductor, at about one or two inches over the electric plate, without touching it, then the pith-balls diverge; or, if they diverged before, they come together, and immediately diverge again with positive electricity.—Remove the hand, and the balls come together; approach the hand, and they diverge, and so on.

If while the pith-balls diverge with negative electricity, I put the metal plate upon the wax, the balls approach each other for a little time, but soon open again with the same, that is with negative electricity.

If, whilst the metallic lies upon the electric plate, I touch the former, the electrometer immediately diverges with positive electricity, which if by touching the electrometer I take off, the electrometer continues without divergence. I touch the metal again, and the electrometer opens again; and so on for a considerable number of times, until the metal plate has acquired its full charge. On taking now the metal plate up, the electro-

meter

meter inftantly diverges with ftrong negative electricity.

I repeated the above experiments with this only difference in the difpofition of the apparatus: I put the electric plate with the excited fealing-wax upon the circular tin-plate, and the glafs uppermoft: and the difference in the refult was, that where the electricity was pofitive in the former difpofition of the apparatus, it now became negative, and *vice verfa*; except that, when I firft lay the electric plate upon the tin plate, the electrometer diverged with negative electricity as well in this as in the other difpofition of the apparatus.

All the above experiments have been repeated with an electric plate, which, befides the fealing-wax coating on one fide, had a ftrong coat of varnifh on the other; and their refult has been fimilar to thofe when the above defcribed plate was ufed.

Experiments on colours.

Having accidentally obferved that an electric fhock, fent over the furface of a card, marked a black ftroke upon a red fpot on the card, I was from this induced to try what would be the effect of fending fhocks over cards painted with different water colours. Accordingly

I painted ſeveral cards with almoſt every colour I had, and ſent ſhocks over them when they were very dry: the effects were as follows[(b)]:

Vermillion was marked with a ſtrong black track, about one-tenth of an inch wide.

Carmine received a faint and ſlender impreſſion, of a purple colour.

Verdigreaſe was ſhaken off from the ſurface of the card.

White lead was marked with a ſtrong black track, not ſo broad as that on vermillion.

Red lead was marked with a faint mark much like carmine.

The other colours I tried were, orpiment, gamboge, ſap-green, red ink, ultramarine, Pruſſian blue, and a few others, which were compounds of the above, but they received no impreſſion.

It having been inſinuated, that the ſtrong black mark, which vermillion receives from the electric ſhock, might poſſibly be owing to the great quantity of ſulphur contained in that mineral, I was induced to make the following experiment. I mixed together equal quantities of orpiment and flower of ſulphur, and with this mix-

(*b*) The force employed was the full charge of one foot and a half of coated glaſs.

ture, by the help, as uſual, of very diluted gum-water, I painted a card; but the electric ſhock ſent over this left not the leaſt impreſſion.

Deſirous of carrying this inveſtigation on colours a little farther, I procured ſome pieces of paper painted on both ſides with oil colours[(c)], and ſending the charge of two feet of coated glaſs over each of them, I obſerved that the pieces of paper painted with lamp-black, Pruſſian blue, vermillion, and purple brown, were torn by the exploſion; but white lead, Naples yellow, Engliſh ochre, and verdigreaſe, remained unhurt.

The ſame ſhock ſent over a piece of paper, painted very thick with lamp-black and oil, left not the leaſt impreſſion. I ſent the ſhock alſo over a piece of paper, unequally painted with purple-brown, and the paper was torn where the paint laid very thin, but remained unhurt where the paint was evidently thicker.

Having repeated thoſe experiments ſeveral times, and with ſome little variation, they were attended with different effects; however, they all ſeem to point out the following propoſition.

I. A coat of oil-paint over any ſubſtance, defends it from the effects of ſuch an electric ſhock as would otherwiſe injure it; but by no means defends it from the force

(c) The colours were mixed with linſeed oil.

 of

of every electric ſhock that can be given. II. No one colour ſeems preferable to the others, if they be equal in ſubſtance, and equally well mixed with oil; but a thick coating does certainly afford a better defence than a thinner one.

By rubbing the above mentioned pieces of paper, I find that the paper painted with lamp-black and oil is more eaſily excited, and acquires a ſtronger electricity than the papers painted with the other colours; and perhaps on this account it may be, that lamp-black and oil might reſiſt the ſhock ſomewhat better than the other paints.

It is remarkable, that vermillion receives the black impreſſion, when painted with oil, nearly as well as when painted with water. The paper painted with white lead and oil receives alſo a black mark, but its nature is very particular. The track, when firſt made, is almoſt as dark as that marked on white lead painted with water; but it gradually loſes its blackneſs, and in about two hours after it appears without any darkneſs, and when the painted paper is laid in a proper light appears only marked with a colourleſs track, as if made by a finger-nail.

Promiſcuous Experiments.

Conſidering what a ſtrong ſpark is obtained from the metal plate belonging to Mr. VOLTA's machine, when not the leaſt ſpark can be obtained from the electric plate itſelf, I was naturally induced to make uſe of the above mentioned metallic plate in diſcovering the electricity of very weak electrics, which otherwiſe would be either in-obſervable, or ſo little as not to permit its quality to be aſcertained. Accordingly, by the uſe of this plate, I obtain a very ſenſible electricity from the hairs of my legs and of my head, or the head of almoſt any other perſon, when ſtroaked.

In this manner I obtain ſo ſtrong ſparks from the back of a cat, a hare's ſkin, a rabbit's ſkin, a piece of paper, or a piece of new flannel, that I can preſently charge a coated phial with either of thoſe, and ſo ſtrongly as to pierce a hole through a card with its diſcharge.

I have often obſerved, that, when ſtroaking a cat with one hand I hold it with the other, I feel frequent ſmart pricklings on different parts of that hand which holds the animal. In theſe circumſtances very pungent ſparks may be drawn from the tips of the ears of the cat.

Smooth glafs rubbed with a rabbit's fkin, dry and warm, acquires, I find, the negative electricity; but if the fkin is cold, the glafs is excited pofitively.

New white flannel has alfo fuch ftrong electric power, that fometimes I have excited fmooth glafs negatively with it.

Confidering the ftrong electric power of new white flannel, I thought that a piece of it rolled round the globe of an electrical machine would perhaps give a ftronger electricity on the prime conductor than the glafs itfelf. In order to try the truth of my fuppofition, I tied a large piece of flannel dry and warm round the globe of the machine; and for a rubber I applied the palm of my hand, then turned the winch, firft flowly, and afterwards brifkly; but, contrary to my expectation, I obferved that the electricity at the prime conductor, although pofitive, was fo weak, that the index of your electrometer was not moved. Surprifed at this event, I refolved to take off the apparatus; but I was more furprifed when, on removing the flannel from the globe, the former appeared fo ftrongly pofitive, that it darted feveral fparks to my arm and other contiguous bodies; and the latter remained fo ftrongly negative, that your electrometer upon the prime conductor inftantly elevated its index to about 45°. I repeated this experiment feve-

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ral times, and the effect was always the ſame. The electricity of the flannel and of the glaſs, therefore, balanced each other.

Having had occaſion to coat a ten-ounce phial, I ſtuck the inſide coating, which was of braſs filings, with varniſh, agreeable to the directions given by ſome writers on electricity. This phial remained about a week unuſed; but it happened, that, whilſt I was charging and diſcharging it for ſome experiments, I obſerved that on making a diſcharge it exploded with a greater noiſe than uſual, the cork with the wire being at the ſame time blown out of the neck of it. Being intent upon the main experiments in hand, I omitted to examine the phenomenon of the phial. I replaced the cork on it, and went on charging and diſcharging it again; but it had not been charged above three or four times more, when I obſerved that, on making a diſcharge, the varniſh that ſtuck to the braſs filings was in a flame, which burned the bottom and ſides of the cork conſiderably, and occaſioned a good deal of ſmoke and flame to come out of the bottle. You will recollect, that I repeated this experiment in the preſence of yourſelf, Mr. ADAMS, and Mr. COVENTRY, when it ſucceeded perfectly; but the varniſh was this time ſo far burnt, that the braſs filings, which

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by the combuſtion had changed their colour, were almoſt all dropped to the bottom of the phial.

I ſhall conclude this paper with the deſcription of a pocket electrometer which I have lately conſtructed, and which, on ſeveral accounts, ſeems preferable to thoſe of the moſt ſenſible ſort now in uſe. The caſe and handle of the electrometer is formed by a glaſs tube about three inches long, and three-tenths of an inch in diameter; half of which is covered with ſealing-wax. From one extremity of this tube, that is, that without ſealing-wax, a ſmall loop of ſilk proceeds, which ſerves occaſionally to hang the electrometer on a pin, &c. To the other extremity of the tube, a cork is adapted, which, being cut tapering on both ends, can fit the mouth of the tube with either extremity. From one extremity of this cork two threads proceed, a little ſhorter than the length of the tube, ſuſpending each a little cone of pith of elder. When this electrometer is to be uſed, that end of the cork which is oppoſite to the threads is puſhed into the mouth of the tube, then the tube forms the inſulated handle of the pith electrometer, as appears in fig. 1. When the electrometer is to be carried in the pocket, then the threads are put into the tube, and the cork ſtops it, as is repreſented in fig. 2. The peculiar

advantages

advantages of this electrometer are, its convenient ſmall ſize, its great ſenſibility, and its continuing longer in good order than any other I have yet ſeen, as you have yourſelf experienced[d]. To preſerve this electrometer from injury, it ſhould be carried in a tooth-pick caſe, or ſome other of the like ſort.

(*d*) I have lately conſtructed a portable electrometer of another kind, which is contrived ſo as not to be affected by the wind or the rain, and conſequently is very convenient to examine the electricity of the clouds out of doors in time of thunder-ſtorms: but I ſhall take another opportunity to preſent to the Royal Society a particular deſcription of the ſame.

Fig: 1.

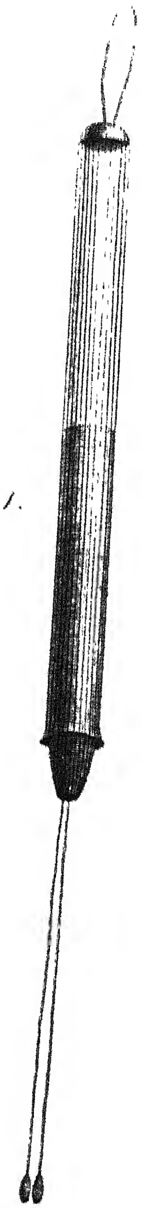

Fig: 2.

XXI. *Barometrical Obſervations on the Depth of the Mines in the* Hartz. *By* John Andrew de Luc, *F. R. S. In a Letter to Sir* John Pringle, *P. R. S.*

S I R,

Pimlico, Feb. 8, 1777.

Read March 20, 1777.

I HAVE the honour to lay before you ſome obſervations on the Barometer, which I made during my ſhort journey into Germany; and beg, if you think them worthy its attention, that you would communicate them to the Royal Society.

I ſat out with the hopes of being able to make a ſhort excurſion into the Hartz, to viſit ſome of the mines there. Theſe I knew were extremely deep; and it made me very deſirous to try in them my rules for meaſuring heights

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Obſervations Barométriques ſur la Profondeur des Mines du Hartz. *Par* Jean Andrée de Luc, *Membre de la Société Royale*, &c. *dans une Lettre au Chevalier Baronet* Pringle, *Preſ. de la Soc. R.*

MONSIEUR,

J'AI l'honneur de vous faire part de quelques obſervations du Barométre, que j'ai faites dans un petit voyage en Allemagne; vous priant de les communiquer à la Société Royale, ſi vous les trouvez dignes de ſon attention.

En partant pour ce voyage, j'avois l'eſpérance de faire une excurſion dans le Hartz pour y viſiter quelques unes de ſes mines. Je ſavois qu'elles étoient fort profondes; et par conſéquent j'avois grande envie d'y eſſayer mes régles

heights by the Barometer, that I might know whether in thoſe pits (where exhalations of all kinds ſpread themſelves) the condenſations of the air follow the ſame laws that they do out of them.

I nearly miſcarried in this intereſting operation through an accident which had happened to my Barometer. Having lent it to a friend, when I came to examine it a little before my ſetting out, I found that air had ſome how got into it. Luckily I had time enough to take it to pieces and make the quickſilver boil in it; a circumſtance which I only mention in order to obſerve, that I ſucceeded ſo thoroughly in this manœuvre, that, from that time to this, the quickſilver has always adhered to the ſummit of the tube as often as I have raiſed it to it, juſt in the ſame manner as it adheres by the ebullition. It requires a ſhake to make it deſcend: and ſometimes

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régles pour la meſure des hauteurs par le Barométre; pour ſavoir ſi dans ces puits, où des exhalaiſons de tant d'eſpéces ſe répandent, les condenſations de l'air ſuivroient les mêmes loix qu'au dehors.

Je faillis à manquer cette intéreſſante opération par un accident arrivé à mon Barométre. Je l'avois prêté; et lorſque je l'examinai à la veille de mon départ, je trouvai qu'on y avoit laiſſé entrer de l'air. J'eus le tems heureuſement de le démonter, et d'y faire bouillir le mercure: circonſtance dont je ne fais mention, que pour ajouter; que je reüſſis ſi parfaitement dans cette opération, que dès ce moment, pendant tout mon voyage, et juſqu'à aujourd'hui, le mercure a toûjours continué de s'attacher au ſommet du tube, lorſque je l'y raméne; comme il s'y attache au moment de l'ebullition: et il n'en deſcend que par une ſecouſſe.

ſometimes the column breaks beneath the ſummit, and there remain only ſome lines of the quickſilver ſuſpended to the top.

It is for barometers cleared of air to this degree that my *formulæ* have been determined: and I had the ſatisfaction to find that they anſwered in the Hartz juſt as they had done upon the mountains in the neighbourhood of Geneva, where they took their origin.

Another remarkable circumſtance which relates to the Barometer itſelf is as follows. Having occaſion for correſponding obſervations in ſome places of my rout, I applied to obſervers who had good Barometers; amongſt which I met with one of Mr. DOLLOND's. Theſe Barometers I compared with my own, being well aſſured beforehand, that I ſhould find a difference in the heights indicated, from the circumſtance of their having ciſterns at

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ſecouſſe. Quelquefois même la colomne ſe romp au deſſous du ſommet, et il ne reſte que quelques lignes de mercure ſuſpendu.

C'eſt pour des Barométres purgés d'air à ce point, que mes formules ont été déterminées: auſſi ont-elles reuſſi dans le Hartz, tout comme dans les montagnes des environs de Genève, où elles ont pris naiſſance.

Voici encore, Monſieur, une circonſtance remarquable, qui regarde le Barométre même. Ayant eu beſoin en quelques endroits de ma route, d'obſervations correſpondentes, je m'adreſſai à des amateurs, qui avoient de bons Barométres. J'en trouvai un entr' autres de M. DOLLOND. Je comparai ces Barométres au mien, étant bien aſſuré de trouver de la différence dans la hauteur indiquée; parcequ'ils avoient des reſervoirs en bas; ce qui fait que la colomne barométrique

at the bottom, which makes the barometrical column always fhorter in thefe than it is in a plain tube in the form of a fyphon, as I have fhewn in my treatife on the *Modifications of the Atmofphere.* Accordingly this was the cafe in all thefe Barometers; they did all ftand lower than mine, but varied from each other, according to particular circumftances, depending chiefly on the diameter of the tube, and the figure of the ciftern.

In going from Hanover to the Hartz I went through Gottingen, where I did not ftop then, becaufe I wifhed to make ufe of the fine weather. I left it therefore without having fettled any thing about correfponding obfervations of the Barometer, Profeffor LICHTENBERG having been kind enough to undertake the care of procuring them for me; the comparifon of the inftruments being deferred till my return. Profeffor ERXLEBEN was accordingly

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trique y eft toûjours plus courte, que dans un tuyau fimple en forme de fyphon; comme je l'ai expliqué dans mon Ouvrage fur les *Modifications de l'Atmofphère.* C'eft auffi ce que je trouvai dans tous ces Barométres: ils fe tenoient tous plus bas que le mien; mais diverfement, fuivant quelques circonftances particulières, dépendantes principalement du diamétre du tube, et de la figure du refervoir.

En allant de Hanovre au Hartz, je paffai par Gottingue, où je ne m'arrêtai point alors, parce que je voulois profiter du beau tems. J'en partis donc, fans avoir rien déterminé pour des obfervations correfpondantes du Barométre; M. le Profeffeur LICHTENBERG ayant bien voulu fe charger du foin de m'en procurer; et renvoyant à mon retour la comparaifon des inftrumens. Il s'adreffa pour cet effet à M. le Profeffeur ERXLEBEN; parce qu'il avoit un Barométre fait d'un fimple tube

ingly applied to by him, becaufe he had a Barometer made of a fingle bent tube upon the principle of mine. During my journey Mr. ERXLEBEN was fo good as to obferve this Barometer very frequently; and it is from his obfervations that the heights of fome of the places of the Hartz, which I fhall mention, have been determined.

At my return, I placed my Barometer near that of Mr. ERXLEBEN; and, when they were expofed to the fame temperature, there was found no difference between them.

This example, joined to all thofe with which my own experience has furnifhed me for a long time paft, makes me more and more folicitous that the lovers of natural philofophy would fix the fcale of their Barometers with cifterns (very convenient ones no doubt for common ufe) by comparing them with a Barometer made in the form of

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tube recourbé, fur la principe du mien. M. ERXLEBEN eut la bonté d'obferver très fréquemment ce Barométre pendant mon voyage; et c'eft d'après fes obfervations, que j'ai déterminé les hauteurs de quelques endroits du Hartz dont je ferai mention.

A mon retour j'apportai mon Barométre auprès de celui M. ERXLEBEN; et quand ils furent réduits à la même température, il ne fe trouva entr'eux aucune différence.

Cet exemple fe joignant à tous ceux que mes propres expériences m'ont fourni depuis long tems, me fait defirer toûjours davantage que les Phyficiens veuillent bien fixer l'echelle de leurs Baromètres à refervoir (très commodes fans doute pour l'ufage ordinaire) en les comparant à un Baromètre fait en forme de fiphon;

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of a ſyphon, and not by any immediate meaſure commencing at the level of the quickſilver in the ciſtern. This is the ſureſt method to render exactly correſpondent thoſe obſervations which are made with Barometers which one cannot compare together; at the ſame time that the barometrical height expreſſed by Barometers of this form is the only true one; that is to ſay, the only one which, after the correction for the heat, expreſſes the weight of the air, by the height of a column of quickſilver of a given denſity, with which it is really in equilibrio.

I ſhall begin the account of my obſervations of the Barometer in the Hartz by thoſe which I made in places, the height of which is aſcertained.

Knowing that the ore is drawn up in pales from the pits of the mines, I thought at firſt that it would be

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et non par une meſure immédiate, qui parte du niveau du mercure dans le reſervoir. C'eſt le plus ſur moyen de rapporter avec exactitude les unes aux autres, des obſervations faites avec des Barométres que l'on n'a pu comparer; en même tems que la hauteur barométrique exprimée par les Barométres de cette forme, eſt la ſeule vraie; c'eſt à dire la ſeule qui, après la correction pour la chaleur, exprime le poids de l'air par la hauteur d'une colomne de mercure de denſité donnée, avec laquelle il eſt réellement en équilibre.

Je commencerai, Monſieur, le récit de mes obſervations du Barométre dans le Hartz, par celles que j'ai faites en des lieux dont la hauteur eſt connuë.

Sachant que l'on monte le minerai dans des ſeaux, par les puits des mines, j'avois crû d'abord qu'il me ſeroit poſſible de meſurer ces profondeurs au cordeau; et

be eaſy to meaſure their depths with a line, and I had accordingly provided myſelf with all the neceſſary implements for that purpoſe; but when I arrived at Clauſthal, the principal place of the King's mines, I found that thoſe pits, being dug in the direction of the veins of ore, are too inclined to make ſuch a mode of menſuration practicable.

At firſt this gave me great concern, becauſe I had my experiments much at heart; but I was ſoon made eaſy by Baron REDEN, captain-general of the mines. "You do not want theſe meaſures," ſaid he, "ſince it is "of much more conſequence to us, than it can poſſibly "be to you, to know exactly the depth of all the points "of theſe mines. Without ſuch knowledge, how could "we direct ourſelves in boring from one to the other?" This conſideration did in fact diſpel all the ſcruples which had made me deſirous of meaſuring the depths myſelf,

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et je m'étois muni des choſes néceſſaires à cet effet. Mais lorſque je fus arrivé à Clauſthal, chef lieu des mines du Roi, j'appris que ces puits, creuſés dans la direction des filons, ſont trop inclinés pour que cette eſpéce de meſure ſoit poſſible. J'y eus d'abord beaucoup de regret; parce que j'avois fort à cœur ces expériences: mais M. le Baron DE REDEN, capitaine-général des mines, me tranquilliſa bientôt. "Vous n'avez pas beſoin de meſurer," me dit-il, "il nous "importe bien plus qu'à vous de connoitre exactement la profondeur de tous "les points de ces mines. Sans cela, comment nous dirigerions-nous, pour "percer de l'une à l'autre?" Cette conſidération en effet, fit diſparoître pleinement

myſelf, and by ſo doing gave me time for a greater number of obſervations.

The firſt of this kind were made in three mines in the environs of Clauſthal; called the Dorothea, the Caroline, and the Benedict. Mr. DE REDEN and Meſſ. HELZENER and FRIEDRICH, chief officers of the miners, went down with me; and, whilſt we were penetrating into the bowels of the mountain, Mr. LEYSER, ſyndic of the mines, and a lover of meteorological obſervations, was obſerving every quarter of an hour, at the top of the pit into which we had deſcended, a barometer and thermometer which have been ſince compared with mine.

I had obſerved my Barometer at the mouth of the Dorothea pit when we were going into the mine at half an hour after eleven in the morning; I obſerved it at

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ment les ſcrupules qui m'avoient fait deſirer de meſurer moi-même ces profondeurs; ce qui me facilita un plus grand nombre d'obſervations.

Les prémiéres de ce genre furent dans trois mines contiguës des environs de Clauſthal; nommées la Dorothée, la Caroline, et la Bénédicte. M. DE REDEN et Meſſ. HELZENER et FRIEDRICH, prémiers officiers des mineurs, ſe donnèrent la peine d'y deſcendre avec moi: et tandis que nous nous enfonçions dans le ſein de la montagne, M. LEYSER, ſindic des mines, et amateur des obſervations météorologiques, obſerva de quart d'heure en quart d'heure, au haut du puits par lequel nous étions deſcendus, un barométre et un thermométre, qui furent depuis comparés aux miens.

J'avois obſervé mon Barométre en entrant dans les mines à $11\frac{1}{2}$ h. du matin, au haut du puits de la Dorothée; je l'obſervai au fond de ce puits à $1\frac{1}{4}$ h.; au fond

at the bottom of this pit at a quarter paſt one o'clock; at the bottom of the Caroline, at a quarter paſt three o'clock; in the loweſt ſearching gallery of the Benedict, at ſix o'clock; and again, at the mouth of the Dorothy, on our return, at ſeven o'clock. During the ſeven hours and a half that we had ſtaid in the mines, the greateſt variation of the Barometer had not exceeded a quarter of a line; and Mr. LEYSER's obſervations indicated the times when this variation had taken place.

At my return I calculated theſe obſervations, and gave the reſults to Baron REDEN, in order that the ſubterraneous Geometer might compare them with the regiſters which are kept of the depths of all theſe mines. The following are the reſults of theſe calculations, the particulars of which I likewiſe ſend you.

The

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fond de celui de la Caroline à $3\frac{1}{4}$ h.; dans la galerie de recherche la plus baſſe de la Benedicte à 6 h.; et enfin je l'obſervai encore à 7 h. etant de retour à l'entrée du puits de la Dorothée. Pendant les $7\frac{1}{2}$ h. que nous étions reſtés dans les mines, ſa plus grande variation avoit été d' $\frac{1}{4}$ ligne; et les obſervations de M. LEYSER me marquoient les tems où cette variation s'étoit faite.

Au retour je calculai ces obſervations, et j'en remis les reſultats à M. le Baron DE REDEN, pour les faire comparer par le Geométre ſouterrain, avec les regitres tenus de toutes les profondeurs dans les mines. Voici, Monſieur, les réſultats de ces calculs, dont j'ai l'honneur de vous envoyer auſſi les détails.

	French toises.
The depth of the Dorothy pit between two fixed points,	168,96
That of the Caroline, relatively to the same point at the mouth,	170,74
That of the lowest searching gallery of the Benedict, relatively to the same point,	143,96

Mr. FRIEDRICH was appointed to give me the geometrical measures. He had been present at my observations, and found the results of them so near those measures, considering they had been furnished by a method so easy, and according to his notions so extraordinary, that he gave me a certificate in due form of the real heights, which are as follows:

The

	Toises de Fr.
La profondeur du puits de la Dorothée, entre deux points fixes,	168,96
Celle du puits de la Caroline, relativement au même point d'enhaut,	170,74
Celle de la galerie de recherche la plus profonde de la Benedicte, de même,	143,96

Ce fut M. FRIEDRICH, qui fut chargé de me communiquer les mesures geométriques. Il avoit été témoin des observations; et il en trouva les résultats si près de ces mesures, pour avoir été fournis par une route si aisée et si singulière à ses yeux, qu'il m'expédia un certificat en duës formes de ces hauteurs réelles: elles étoient comme suit:

Le

	Lachters.
The depth of the Dorothy, from the points of obſervation,	172,31
That of the Caroline,	173,92
The gallery of the Benedict,	144,79

I could not at firſt judge of the agreement of the two meaſures, becauſe it was neceſſary to know the relation between the Lachter and the French Toiſe. I had brought with me a very accurate half Toiſe, which we compared with the half Lachter, and found the latter ſhorter than the former in the proportion of 61 to 62.

Reducing then, according to this proportion, the geometrical meaſures above mentioned into French toiſes, we ſhall have,

The

	Lachters, ou toiſes du Hartz.
Le puits de la Dorothée en partant des points des obſervations, —	172,31
Celui de la Caroline, — — —	173,92
La galerie de la Benedicte, — — —	144,79

Je ne pus pas juger d'abord du rapport des deux meſures, parcequ'il falloit connoitre celui de la Lachter avec la Toiſe de France. J'avois apporté avec moi une demi Toiſe fort exacte; nous la comparâmes à la demi Lachter, et nous trouvâmes celle-ci plus courte que la demi Toiſe dans le rapport de 61 à 62.

En reduiſant donc ſuivant ce rapport, les meſures geométriques ci-deſſus en Toiſes de France, nous aurons:

	French toifes.
The pit of the Dorothy,	169,53
That of the Caroline,	171,12
The gallery of the Benedict,	142,42

The geometrical meafures come then very near the barometrical ones, for they only differ by

	Toifes.
In the firft obfervation,	0,57 fhort.
In the fecond,	0,38 fhort.
In the third,	1,54 excefs.

I was really furprized to have come fo near to the geometrical meafures (which, as I fhall have occafion to mention hereafter, may be looked upon as the real heights) for I had imagined, that the exhalations of all kinds

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	Toifes de Fr.
Le puits de la Dorothée, — — —	169.53
Celui de la Caroline, — —	171,12
La galerie de la Benedicte, — — —	142,42

Les mefures geométriques s'approchent alors de bien près des mefures barométriques; puifque celles-ci différent feulement des autres favoir:

	Toife.
Dans la prémiere obfervation de,	0,57 en défaut.
Dans la feconde de,	0,38 auffi en défaut.
Dans la troifieme de,	1,54 en excés.

Je fus réellement furpris d'avoir approché de fi près des mefures geométriques; qui, comme j'aurai occafion de le dire enfuite, peuvent être regardées comme les hauteurs réelles. Car j'avois imaginé, que les exhalaifons de toute efpéce qui

kinds in the mines muſt in ſome meaſure affect the common laws of the air's claſticity in different degrees of heat, if not its abſolute elaſticity. On reflecting, however, on this ſingular conformity of the air in mines with the external air, I ſoon found the cauſe of it in the extreme care taken to procure a circulation of external air in the mines, in order to prevent the pernicious effects of the exhalations: ſo that the ſame means, which really preſerve the health of the miners in their ſubterraneous abodes, give the air which circulates in them, and more eſpecially that of the pits in which are the principal currents, the properties of the external air as to barometrical meaſurements. Doubtleſs this is the cauſe of that intereſting phenomenon, as ſatisfactory for the ſecurity it gives to the lives of the miners, as for the application of the laws of aërometry: this was again confirmed by obſervations I made ſome days after in other mines,

qui ſe répandent dans les mines, devoient y altérer les loix communes de l'élaſticité de l'air en différens degrés de chaleur; et peut-être ſon élaſticité abſoluë. Mais en réfléchiſſant enſuite ſur cette ſinguliére conformité de l'air des mines, avec l'air extérieur, j'en apperçus la cauſe dans le ſoin extrème qu'on prend d'y faire circuler l'air extérieur, pour empêcher les mauvais effets des exhalaiſons. Ainſi les mèmes moyens qui conſervent réellement la ſanté des mineurs dans leurs demeures ſouterraines, donnent à l'air qui y circule, et ſurtout dans les puits, où ſont les principaux courants, les propriëtés de l'air extérieur dans les meſures barométriques. C'eſt là ſans doute la cauſe de cet intéreſſant phénoméne, auſſi tranquilliſant ſur le ſort des mineurs, que ſur l'application des régles d'aërométrie. Ce qui ſe confirme encore par d'autres obſervations que je fis quelques

mines, where indeed I met with fome irregularities, but not fuch as might have been expected from barely confidering the local circumftances.

Thefe mines are in the Ramelfberg near Goflar. The ore that is chiefly extracted from them, as well as from thofe of Claufthal, is lead; but they are worked in another manner. The vein of ore, which is near eighteen toifes broad, is extremely impregnated with pyrites: infomuch that, when you heat it, the vapour of the fulphur, which difengages itfelf, burfts the ftone, which falls down in large fragments. The method then is to light great fires againft the rock; and, when they are extinguifhed, the miners affift with their inftruments the fall of the ftones, that may ftill remain fufpended.

Sulphureous vapours, therefore, conftantly difengaged from the heated ore, circulate in the caverns of the mountain,

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quelques jours après dans d'autres mines, où je trouvai quelque irrégularité, mais non point fuivant ce que les circonftances locales fembloient devoir en produire.

Ces mines font dans le Ramelfberg, près de Goflar. Elles fourniffent principalement du plomb, comme celles de Claufthal; mais on les exploite d'une autre maniére. Le filon, qui a près de 18 toifes de largeur, eft extrèmement pénètré de pyrite; tellement qu'en l'échauffant, les vapeurs du fouffre qui fe dégage, font crévaffer la pierre, qui tombe d'elle-même en grand lambeaux. On allume donc de grand feux contre le rocher; et lorfqu'ils font éteints, les mineurs aident avec des inftrumens, la chute des pierres qui font encore fufpendues.

Il fe détache donc prefque conftamment du minerai échauffé, des vapeurs fulfureufes, qui circulent dans les cavernes de la montagne, et dans les puits et foupiraux

mountain, and in the vents and pits by which they communicate with each other. The day I entered them, was a day of reſt for the miners; and there was no other fire but that which Mr. ROEDER, their principal, was kind enough to order to be lighted, that I might form an idea of this method of working mines. Notwithſtanding this, I could perceive here and there ſulphureous vapours, which in ſome places were even ſtrong enough to occaſion a very troubleſome ſenſation of ſuffocation. In ſome places I felt the remainder of the heat communicated to the rock from whence theſe vapours were exhaled; and in ſome caverns, where the fire had only been put out the evening before, FAHRENHEIT's thermometer roſe to 110°. But this very heat is a moſt powerful ventilator, as it makes the external air circulate in theſe mines. Indeed the currents of air are ſo rapid in them, that one is obliged to have

ſoupiraux par leſquels ces cavernes communiquent les unes aux autres. Le jour que j'y entrai étant un jour de repos pour les mineurs, il n'y eut de feu dans les mines, que celui que Mr. ROEDER leur chef eut l'honnêteté de faire allumer pour me donner une idée de cette exploitation. Cependant j'appercevois ça et là des vapeurs de ſouffre; et ſouvent même elles étoient aſſez fortes, pour m'occaſioner un ſentiment de ſuffocation très pènible. Quelqueſois auſſi j'éprouvois les reſtes de la chaleur communiquée au rocher d'où ces vapeurs s'exhaloient: et dans quelques cavernes où le feu n'étoit éteint que dès la veille, le thermomètre de FAHRENHEIT, monta juſqu'à 110°: mais cette chaleur même eſt un ventillateur très puiſſant, pour faire circuler l'air extérieur dans ces mines. Auſſi les courants d'air y ſont-ils ſi rapides, qu'on eſt obligé d'avoir des portes à

have doors at the entrance of each gallery, and ſometimes many of them one after an other, without which it would not be poſſible to keep any lamps lighted in theſe ſubterraneous regions.

It is probably to this conſtant renewal of the air, that the miners of Ramelſberg are indebted for the good health they enjoy, notwithſtanding the prodigious heat they feel while they are at work, and the quantity of ſulphur which exhales from every part; and it is likewiſe probably from this cauſe that my barometrical obſervations gave me the heights more exactly than I could have expected from theſe circumſtances. The following are the reſults of theſe obſervations, the particulars of which you likewiſe have ſubjoined.

Height

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l'entrée de toutes les galeries; et quelquefois même pluſieurs de ſuite; ſans quoi il ne ſeroit pas poſſible de tenir les lampes allumées dans ces ſouterrains.

C'eſt ſans doute à ce renouvellement continuel de l'air, que les mineurs du Ramelſberg doivent la bonne ſanté dont ils jouiſſent, malgré la chaleur prodigieuſe qu'ils éprouvent pendant le tems de leur travail, et la quantité de ſouffre qui s'exhale de toute part: et c'eſt auſſi probablement la cauſe, de ce que mes obſervations du Barométre, me donnèrent les hauteurs, plus exactement que je ne l'attendois d'après ces circonſtances. Voici, Monſieur, les réſultats de ces obſervations, dont vous avez auſſi les détails ci-joint.

Hauteur

	French toifes.
Height of the gallery of Breitling, above the bottom of the pit of Kaunkuhl,	44,41
Height of the entry of the mines, above the gallery of Breitling,	27,04
Height of the top of the pit of Kaunkuhl, above the entry of the mines, by external obfervations,	41,27
Depth of the pit of Kaunkuhl, meafured in three parts, one of them without the mines,	112,72
Depth of the fame pit, determined by immediate obfervations made at the top and the bottom,	113,13

I could not get the geometrical meafures the fame day, becaufe there was not time enough left to look for them

	Toif. de Fr.
Hauteur de la galerie de Breitling, fur le fond de puits de Kaunkühl,	44,41
Hauteur de l'entrée des mines, fur la galerie de Breitling,	27,04
Hauteur du haut du puits de Kaunkühl, fur l'entrée des mines, par des obfervations extérieures,	41,27
Profondeur du puits de Kaunkühl, mefuré en trois portions, dont une à l'extérieur des mines,	112,72
Profondeur du même puits, déterminée par des obfervations immédiates, au fond et au haut,	113,13

Je ne pus pas avoir d'abord les mefures geométriques; parce qu'il ne refta pas affez

them then. But the day after Mr. ROEDER ſent them to Mr. DE USLER, Comptroller of the treaſure, who had been kind enough to accompany me to Ramelſberg, and in all my ſubterraneous rambles. Mr. ROEDER likewiſe came with us; he took notes of the places where the obſervations were made, and ſent the following meaſures, which I have reduced into French toiſes.

	French toiſes.
Height of the gallery of Breitling, above the bottom of the pit of Kaunkuhl,	46,86
Height of the entry into the mines, above the gallery of Breitling,	25,76
Height of the top of the pit of Kaunkuhl, above the entry of the mines,	41,32
	113,94

Hence

aſſez de tems pour les chercher le même jour. Mais dès le lendemain M. ROEDER les envoya à M. DE USLER, Controleur du tréſor, qui avoit eu la bonté de me conduire au Ramelſberg et dans toute ma route ſouterraine. M. ROEDER nous y avoit accompagnés; il avoit pris note des lieux où s'étoient faites les obſervations; et il envoya les meſures ci-après, que j'ai ſeulement changées en Toiſes de France.

Hauteur de la galerie de Breitling, ſur le fond du puits de Kaunkühl,	46,86
Hauteur de l'entrée des Mines ſur la galerie de Breitling,	25,76
Hauteur du haut du puits de Kaunkühl ſur l'entrée des mines,	41,32
	113,94

Il

Hence it follows, that in the meaſurement of the total height of the pit, the barometrical meaſure differed from the geometrical one by 0,81 toiſe, or about $\frac{1}{140}$ in defect; that in the meaſurement of a part only of this height, made without the mine, it differed only by 0,05, or about $\frac{1}{800}$, likewiſe in defect; but that in the two other portions of the height, taken within the mine, it differed in the one by $\frac{1}{18}$ in defect, and in the other by $\frac{1}{21}$ in exceſs: and hence we obſerve, that the abſolute errors are only of $2\frac{1}{2}$ toiſes and $1\frac{1}{4}$ toiſe; and that theſe ſmall differences may have ariſen from ſome fault in the obſervation, as well in ſmall heights as in greater ones: and in this caſe, where the errors are in defect and exceſs, it is probable that they ariſe from ſome ſuch cauſe; and that the ſulphureous vapours have little or nothing to do with them.

Having

Il reſulte de là, que ſur la hauteur totale du puits, la meſure barométrique a différé de la meſure geométrique, de 0,81 toiſe, ſoit d'environ $\frac{1}{140}$, en défaut; que dans la meſure d'une partie de cette hauteur, faite en dehors des mines, elle n'a différé que de 0,05, ſoit d'environ $\frac{1}{800}$, auſſi en défaut: mais que dans les deux autres portions de la hauteur, priſe ſans l'intérieur de la mine, elle a différé dans l'une d' $\frac{1}{18}$ en défaut, et dans l'autre d' $\frac{1}{21}$ en excés. Sur quoi il faut remarquer; que les erreurs abſoluës ne ſont que de $2\frac{1}{2}$ toiſes, et d' $1\frac{1}{4}$ toiſe; et que ces pétites différences peuvent reſulter de quelque défaut dans l'obſervation, auſſi bien ſur de petites, que ſur de grandes hauteurs. Et dans ce cas-ci, où les erreurs ſont en excés et en défaut, il eſt bien probable qu'elles tiennent à cela, et que les vapeurs ſulfureuſes n'y entrent pour rien de ſenſible.

Aprés

Having made theſe experiments within the mines, I was deſirous of making ſome in the open air, which I had ſoon a very agreeable opportunity of doing; for having communicated my deſire to Mr. REDEN, he and Mr. RAUSCH, the principal of the ſubterraneous geometers, were ſo good as to be of the party. The latter had had occaſion to determine moſt accurately the height of two external points of the Hartz, relatively to the mines of Clauſthal and Zellerfeld. Nothing more, therefore, was required but to obſerve the Barometer at the entry of a certain mine, which was a fixed point, and to obſerve it again at theſe two external points; one of which was about 3000 toiſes horizontal diſtance, beyond a ſmall hill; and the other, 5000 toiſes off, entirely without the Hartz.

We

Après avoir fait ces expériences dans l'intérieur des mines, je deſirois beaucoup d'en faire auſſi en plein air. L'ayant témoigné à M. DE REDEN, il m'en fournit un moyen très agréable; car lui même, et M. RAUSCH chef des geométres ſouterrains, furent de la partie. Ce dernier avoit eu beſoin, à l'occaſion d'un projet de galerie d'écoulement, de déterminer avec la plus grande exactitude la hauteur de deux points extérieurs au Hartz, rélativement aux mines de Clauſthal et de Zellerfeld. Il ne s'agiſſoit donc que de faire l'obſervation du Barométre à l'entrée d'une certaine mine, qui étoit un point fixe; et de l'aller faire enſuite à ces deux points extérieurs; dont l'un étoit à environ 3000 toiſes de diſtance horizontale, au de là d'une colline, et l'autre à 5000 toiſes, entiérement au dehors du Hartz.

S Nous

We carried this project into execution on the 30th of October, when I found the following heights by the calculations hereunto annexed.

	French toises.
Height of the entry of the mine, called Alte Seegen, above a certain point in the valley of Bremeke,	102,18
Height of the entry of the same mine, above another point near Lasfelde, in the valley of Osterode,	173,81

After I had calculated these observations, Mr. RAUSCH was so good as to give me a profile of our rout, on which the points above mentioned were marked. Their heights, reduced to French toises, are as follow:

The

Nous exécutâmes ce projet le 30 Octobre; et je trouvai les hauteurs suivantes par les calculs ci joints de mes observations.

	Toises de Fr.
Hauteur de l'entrée de la mine, nommée Alte Seegen, au dessus d'un certain point dans la vallée de Bremeke,	102,18
Hauteur de la même entrée de mine, au dessus d'un autre point près de Lasfelde, dans la vallée d'Osterode,	173,81

Après que j'eus calculé ces observations, M. RAUSCH eut la bonté de me donner un profil de la route que nous venions de faire, où les points ci-dessus étoient marqués. Leur hauteur, reduite en Toises de France, est comme suit.

Be

The point of the valley of Bremeke below Alte Seegen,	100,85
That of the vale of Osterode,	173,56

Consequently, one of these barometrical measures, taken in open air, was found to agree very nearly with the geometrical measure; and the other differs only $1\frac{1}{3}$ toise in excess.

The only thing that remained to be enquired into was, whether the geometrical measures could really be relied on; but I soon found that I might trust to the importance they were of to the miners, as well as to the daily experience of the truth of them; although they be made in so very singular a manner, that one does stand in need of this experience, to be persuaded of their exactness.

A twisted

Le point de la vallée de Bremeke, au dessous d'Alte Seegen,	100,85
Celui de la vallée d'Osterode,	173,56

Ainsi, l'une de ces mesures barométriques faites en plein air, s'est trouvée presqu' entiérement semblable à la mesure geométrique; l'autre n'en différe que d'une toise et un tier en excès.

Il ne s'agissoit plus que d'examiner, si les mésures geométriques étoient vraiment dignes de confiance. Mais je vis bientôt, que je pouvois me reposer à cet égard sur l'importance dont il est pour les mineurs qu'elles le soyent; et sur l'expérience qui les vérifie tous les jours. Cependant elles s'exécutent d'une manière si singuliére, qu'il faut réellement cette expérience, pour se persuader qu'elles sont exactes.

Un

A twiſted braſs wire five toiſes long, two puncheons, a ſemi-circle, and a compaſs, are all the inſtruments made uſe of by the ſubterraneous Geometer. By means of his two puncheons, he extends his wire in the direction of the way which he is meaſuring: and by practice he acquires a habit of always ſtretching it to the ſame degree. His ſemi-circle, which is very light, being ſuſpended at the middle of the wire, ſhews him its inclination. By this means he has a right-angled triangle, of which the hypothenuſe and angle at the baſe are known. He has conſequently the vertical height and horizontal diſtance gone over. After this he ſuſpends his compaſs to the wire, in order to find out its declination, and conſequently the direction of his horizontal line. It is in this manner that he draws the plan and ſection of theſe ſubterraneous labyrinths. It is likewiſe by this means that he goes over hills and vales, in

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Un fil de leton tordu de 5 toiſes, deux poinçons, un demi-cercle, et une bouſſole, ſont tous les inſtrumens du Geométre ſouterrain. Il étend ſon fil, par le moyen de ſes deux poinçons, dans la direction du trajet qu'il meſure; l'habitude le lui fait tendre toûjours au même degré. Son demi-cercle, qui eſt fort léger, étant ſuſpendu au milieu de ce fil, lui en montre l'inclinaiſon; il a par ce moyen un triangle rectangle, dont l'hypothenuſe et l'angle ſur la baſe lui ſont connus: il a donc la hauteur verticale et la diſtance horizontale parcouruës. Il ſuſpend enſuite ſa bouſſole au même fil, pour en connoitre la déclinaiſon, et par conſéquent la direction de ſa ligne horizontale. C'eſt ainſi qu'il tire le plan et la coupe de ces labyrinthes ſouterrains: et c'eſt ainſi encore qu'il va chercher au dehors

in order to determine points correſponding to his pits and galleries.

But is this a method that may ſafely be depended upon? The fact anſwers, and ſaves us the trouble of long reaſonings. A miner, ſolely upon the faith of his Geometer, and in the abſolute obſcurity of the entrails of the earth, undertakes a labour that is to coſt him years, in daily boring through a rock. Another miner ſets out to meet him from ſome other mine, or from without. At the end of a determined meaſure, the Gnomes begin to hear each other, and at length they meet. I have obſerved ſome of theſe points of rencounter in the galleries; it is ſometimes difficult to perceive the ſmall winding which has been neceſſary for their meeting end to end.

It

dehors, à travers les vallées et les collines, des points correſpondants à ſes galeries et à ſes puits.

Eſt-ce donc là une méthode dans laquelle on puiſſe vraiment prendre confiance? Le fait parle ici, et épargne les raiſonnemens. Le mineur, ſur la foy de ſon Géométre, s'avanture à entreprendre, dans l'abſoluë nuit des entrailles de la terre, un travail qui lui coutera des années, en perçant journellement le rocher. On vient à ſa rencontre, de quelqu'autre mine, ou du dehors. Au bout de la meſure determinée, nos Gnomes viennent à s'entendre; et enfin ils ſe trouvent. J'ai vu pluſieurs de ces points de rencontre dans les galeries; on à peine quelquefois à apperçevoir le petit évaſement qu'il a fallu faire, pour qu'elles ſe [illegible]nent bout à bout.

Il

It remains, SIR, that I communicate to you ſome other barometrical obſervations, not verified by geometrical ſurvey; by which I have determined the height of ſome points of the Hartz relatively to the plain, and chiefly the higheſt point.

This greateſt elevation, called the Blockſberg or Brocken, is ſituated in the eſtate of Count DE VERNIGUERODE. It was my firſt excurſion on my arrival at the Hartz, and I made it in the company of Mr. REDEN. We ſat out from Clauſthal at ten o'clock, and arrived at Oder-bruck, a hamlet ſituated at the foot of the Brocken, at half an hour after two in the morning. We intended ſetting out at day-break, in order to reach the top of the mountain by ſun-riſe, becauſe that is the moſt favourable moment for ſeeing the great extent of country, which is diſcoverable from this eminence; the vapours, which by degrees tarniſh the picture, not having yet riſen. On this

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Il me reſte à vous communiquer, Monſieur, d'autres meſures barométriques, non vérifiées, par leſqu'elles j'ai déterminé la hauteur de quelques points du Hartz relativement à la plaine, et principalement le plus haut point.

Cette ſommité la plus élevée, nommée le Blockſberg ou Brocken, eſt ſituée dans les terres de M. le Comte DE VERNIGUERODE. Cet fut ma premiére courſe lorſque j'arrivai au Hartz; et M. le Baron DE REDEN la fit déja avec moi. Nous partimes à 10 h. du ſoir de Clauſthaul, et nous arrivâmes à $2\frac{1}{2}$ h. du matin à Oder-brucke, hameau ſitué au pied du Brocken. Notre intention étoit de nous mettre en marche à la pointe du jour, pour arriver au lever du ſoleil au ſommet de la montagne; parceque c'eſt le moment le plus favorable pour voir l'immenſe pays que l'on decouvre de cette hauteur: les vapeurs qui peu à peu terniſſent le tableau,

this occaſion, however, other vapours rendered our nocturnal courſe ineffectual. Clouds reſted on the top of the mountain, and we heſitated a long time whether or not we ſhould put off the party to another day. At length a ray of hope having broke forth at Oder-brucke, we determined to ſet out for the mountain. It was then nine o'clock in the morning, and, had we had fair weather, an hour would have been ſufficient to have carried us to the top; but the clouds growing thicker and thicker, we loſt our way, notwithſtanding we had guides; and, had it not been for a map and compaſs which M. DE REDEN had provided himſelf with, we ſhould not probably have arrived at the top of the Brocken that day, having been for ſome hours wandering round it, without drawing nearer to it. We did at length reach it at noon, being ourſelves as well as the mountain covered with iſicles. The wind was

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tableau, n'étant pas encore élevées. Mais d'autres vapeurs rendirent inutile notre courſe de nuit: les nuages s'emparérent du haut des montagnes; et longtems nous héſitâmes ſi nous ne renverrions pas la partie à un autre jour. Mais enfin un rayon d'eſpèrance ayant luit à Oder-brucke, nous nous déterminâmes à partir pour la montagne. Il etoit 9 h. du matin; et une heure eût ſuffit pour nous rendre au ſommet, ſi nous avions eu beau tems. Mais les nuages s'etant épaiſſis toûjours d'avantage, nous nous égarâmes malgré nos guides; et ſans une bouſſole et une carte de ces montagnes dont M. DE REDEN avoit eu la précaution de ſe munir, nous ne ſerions peut-être pas arrivés de tout le jour au Brocken, autour duquel nous tournions, ſans le trouver. Il étoit midy lorſqu'enfin nous l'atteignimes; étant nous mêmes couvers de verglas, comme toute la montagne. Il faiſoit

was very high, the thermometer ſtood at $31\frac{1}{2}$ of FAHRENHEIT; and the clouds, chaſed by the wind, covered every thing with a cruſt of ice that formed itſelf perceptibly to the eye.

I had taken with me a new hygrometer, conſtructed upon the principles of that I had the honour to preſent to the Royal Society four years ago, but corrected in many things where experience had taught me better. I was in hopes, by taking it to the Hartz, of being able to repeat the experiment of the great dryneſs of the ſuperior regions of the air, which I had made formerly on the Alps; but it happened quite otherwiſe, and I was not ſorry for the contraſt. Being arrived at the top of the Brocken, I ſuſpended my hygrometer on the outſide of a ſmall hut, which M. DE VERNIGUERODE has had the humanity to erect for the benefit of ſuch obſervers as ſhould come

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faiſoit un vent aſſez fort, le thermométre étoit à $31\frac{1}{2}$ de FAHRENHEIT; et les nuages chariés par le vent, couvroient tout d'une croute de glace, qui ſe formoit à vuë d'oeil.

J'avois porté avec moi un nouvel hygrométre, conſtruit ſur les principes de celui que j'ai eu l'honneur de préſenter à la Societé Royale il y a quatre ans; mais où j'ai corrigé pluſieurs des défauts que l'experience m'a déja fait découvrir dans ma prémiére tentative. J'eſpérois en le portant au Hartz, de répéter l'expérience de grande ſécheresſe des couches ſupérieures de l'air, que j'avois faite ci devant ſur l'une des ſommités des Alpes. Mais il en arriva tout autrement: et je ne fus pas fâché du contraſte. Arrivés donc enfin au ſommet du Brocken, je ſuſpendis mon hygrométre au dehors d'une petite hute, que M. DE VERNIGUERODE a eu l'humanité de faire bâtir pour ſervir de refuge aux curieux dans le mauvais

come there in bad weather. In an inſtant the ivory, of which this new inſtrument as well as the former is conſtructed, was covered with iſicles; and what merits ſome attention as to the matter of the *humor*, the hygrometer was by that means brought very nearly down to the point of extreme humidity.

I ſhall not here detain you with accounts of ſeveral other obſervations made with this inſtrument in the courſe of my journey. I am much better pleaſed with it than with the firſt, for many reaſons; however, I have not got over all the difficulties, but luckily alſo I have not exhauſted all the reſources.

I likewiſe obſerved the Barometer on this eminence; I had done it at ſetting out from Oder-brucke, and did it again on my return. I had likewiſe correſponding obſervations at Clauſthal, and during the whole of my journey I had

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mauvais tems. En un inſtant l'yvoire, dont ce nouvel inſtrument eſt fait comme le prémier, fut couvert d'une couche de verglas; et ce qui mérite quelque attention dans la matierè de *l'humor*, l'hygrométre fut réduit par là à très peu près au point de l'humidité extrême.

Je ne m'arràterai pas ici ſur quelques autres obſervations que j'ai faites avec cet inſtrument pendant mon voyage. J'en ai été plus content que du prémier à divers égards. Cependant je ne ſuis pas au bout des difficultés: mais heureuſement non plus, je ne ſuis pas au bout des reſources.

Je fis auſſi l'obſervation du barométre ſur cette hauteur. Je l'avois faite en partant d'Oder-brucke, et je la fis au retour. J'avois auſſi des obſervations correſpondantes à Clauſthal: j'en eus pendant tout mon voyage de journaillères, à

I had daily ones regiſtered at Clauſthal, at Gottingen, and at Hanover. From all theſe obſervations I computed the height of Oder-brucke above all the above-mentioned places, by calculations which I have annexed.

	French toiſes.
The ſmall hut, at the ſummit of the Brocken above Oder-brucke,	172,93
Oder-brucke above Clauſthal,	91,39
Clauſthal above Gottingen by 17 obſervations correſponding as to time, between Profeſſor ERXLEBEN and myſelf,	210,21
Gottingen above Hanover by 16 ſimilar obſervations, between Mr. ERXLEBEN and M. DE HINUBER,	56,45
Total elevation of the Brocken above Hanover,	530,98

It

Clauſthal, à Gottingue, et à Hanovre. De toutes ces obſervations j'ai conclu la hauteur du Brocken ſur tous les lieux ci-deſſus, par les calculs ci-joints, dont voici l'extrait.

	Toiſes de Fr.
La maiſonette au ſommet du Brocken, ſur Oder-brucke,	172,93
Oder-brucke ſur Clauſthal,	91,39
Clauſthal ſur Gottingue, par 17 obſervations correſpondantes pour le tems, entre M. le Profeſſeur ERXLEBEN et moi,	210,21
Gottingue ſur Hanovre, par 16 ſemblables obſervations entre M. ERXLEBEN et M. DE HINUBER,	56,45
Hauteur totale du Brocken ſur Hanovre,	530,98

Il

It will be eafy to come at the elevation of Hanover above the level of the fea, in order to compleat this meafurement: correfponding obfervations of the Barometer will be fufficient for that purpofe. In the mean time it is eafy to eftimate either by the Barometer itfelf (the mean height of which during the month of October was 30,1 Englifh inches on a fecond floor) or by the fmall declivity of the rivers between Hanover and the fea, that the elevation of Hanover above that level is not very confiderable.

Such, SIR, are the moft interefting obfervations of this kind which I have been able to make in the Hartz. It appears to me, that they are of a nature that fhould encourage us to examine more and more into the phyfical principles on which they depend: principles, the extenfive confequences of which promife new fteps in our inveftigation of nature, not only upon our globe but alfo in the heavens.

A De-

Il fera aifé de favoir la hauteur de Hanovre fur le niveau de la mer, pour completter cette mefure. Des obfervations correfpondantes du barométre, fuffiront pour cela. Mais en attendant il eft aifé de juger, foit par le barométre lui même, dont la hauteur moyenne le matin, pendant le mois d'Octobre, fut 30,1 pouces Anglois à un feconde etage; foit par le peu de pente des riviéres jufques à la mer; que l'élevation de Hanovre au deffus de fon niveau n'eft pas bien grande.

Voilà, monfieur, les obfervations les plus intéreffantes de ce genre, que j'aie faites dans le Hartz. Il me femble qu'elles font propres à donner le courage d'examiner de plus près tous les principes phyfiques fur lefquels elles fe fondent; principes, dont les conféquences très étenduës, nous promettent de nouveaux pas dans l'étude de la nature, non feulement fur la terre, mais dans le ciel.

Defcription

A Detail and Calculations of Barometrical Obſervations made at the Hartz *in* October 1776.

Obſervations in the Mines of Clauſthal.

October 26, 1776, at twenty minutes paſt eleven in the morning, I obſerved my Barometer at the entrance of the pit of the mine called Dorothy, and found it (after the allowance made for the heat both in this and all the following obſervations) 26 in. 4. l. $\frac{1}{16}$ French meaſure.

Syndic LEYSER's Barometer being placed near mine, and obſerved every quarter of an hour throughout the whole day, fell a quarter of a line, between half and three quarters of an hour after twelve, and did not vary ſenſibly till four o'clock. It then had a tendency to riſe, for it was ſtationary, though the Thermometer fell; ſo that, on coming

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Deſcription et Calculs des Obſervations du Baromètre faites au Hartz, *en* October 1776.

Obſervations dans les Mines de Clauſthal.

Le 26 Octobre, 1776, à 11 h. 20 m. du matin, j'obſervai mon baromètre à l'entrée du puits de la mine nommée Dorothée; et je le trouvai (la correction étant faite pour la chaleur ici, et dans toutes les obſervations ſuivantes) 26 p. 4 l. $\frac{1}{16}$ de France.

Le baromètre le M. le Sindic LEYSER étant placé auprès du mien, et obſervé enſuite pendant tout le jour de quart d'heure en quart d'heure, deſcendit d' $\frac{1}{4}$ de ligne de midi $\frac{1}{2}$ à midi $\frac{3}{4}$; et ne varia plus ſenſiblement juſqu'à 4 heures. Alors il tendit à remonter; car il fut ſtationaire quoique le thermomètre baiſſait: tellement

coming out of the mines, my Barometer, after the correction for the Thermometer, was much at the ſame point as when I went in.

I had not made any obſervations in the mines before this firſt variation, ſo I will look upon 26 in. 3 l. $\frac{13}{16}$ as being the height of the Barometer at the top of the pit of the Dorothy during my obſervations in the internal part of the mines till three o'clock. But at ſix o'clock I ſhall ſuppoſe it at 26 in. $3\frac{15}{16}$.

At

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tellement qu'au ſortir des mines mon baromètre, après la correction pour le thermomètre, ſe trouva preſque au même point que lorſque nous y étions entrés.

Je n'avois point encore obſervé dans les mines avant le tems de cette prémiere variation: ainſi je regarderai 26 p. 3 l. $\frac{13}{16}$ comme étant la hauteur du baromètre au haut du puits de la Dorothée pendant mes obſervations dans l'intérieur des mines juſqu'à $3\frac{1}{2}$ h. Mais à 6 h. je la ſuppoſerai à 26 p. $3\frac{15}{16}$.

A 1 h.

	In.	L.	16ths of Lines.	Log.
At $1\frac{1}{4}$ h. at two feet from the bottom of the well of the Dorothy,	27	4	$\frac{12}{16}$ = 5260	37209,857
At the top of the pit,	26	3	13 = 5053	37035,493
Difference, — —				174,364
The thermometer on the outside of the building which is over the mine was at −22 of my scale for the dilatations of the air; but this external temperature reached a very little way into the mine. At the bottom of the pit the thermometer was −15. I reckon the mean temperature −15½, and consequently I am to deduct $\frac{31}{1000}$ from the height found by the log, — — —				5,405
Depth of the Dorothy pit in French toises,				168,959

At

	P.	L.	16 de Lig.	Log.
A $1\frac{1}{4}$ h. à 2 pieds du fond actuel du puits de la Dorothée, — — —	27	4	$\frac{12}{16}$ = 5260	37209,857
Au haut du puits, — —	26	3	13 = 5053	37035,493
Difference, — — —				174,364
Le thermométre au dehors du batiment qui couvre la mine étoit à —22 de mon echelle pour les dilatations de l'air. Mais cette température extérieure s'étendoit bien peu avant dans la mine. Au fond du puits, le thermométre étoit —15. Je suppose la température moyenne —15½, et en conséquence je dois deduire $\frac{31}{1000}$ de la hauteur trouvée par les log, — — —				5,405
Profondeur du puits de la Dorothée, en toises de France, —				168.959

	In.	L.	16th of Lines.	Log.
At $3\frac{1}{2}$ h. at a foot and a half from the bottom of the last ladder of the Caroline, -	27	4	13 = 5261	37210,683
At the top of the pit of the Dorothy, -	26	3	13 = 5053	37035,493
Difference, — —				175,190
In the pit of this mine my thermometer stood at −11 of the scale for the dilatations of the air. But the outside thermometer being then at $-23\frac{3}{4}$, I reckon the mean temperature at −13, and consequently I deduct $\frac{26}{1000}$, -				4,455
The bottom of the pit of the Caroline below the entrance of the pit of the Dorothy, — — — —				170,735

At

	P.	L.	16 de Lig.	Log.
A $3\frac{1}{2}$ h. a $1\frac{1}{2}$ pied du bas de la dern. echelle de la Caroline, — —	27	4	13 = 5261	37210,683
Au haut du puits de la Dorothée, —	26	3	13 = 5053	37035,493
Difference, — — —				175,190
Dans le puits de cette mine, mon thermométre se tenoit à —11 de l'echelle pour les dilatations de l'air. Mais le thermométre étant alors au dehors à $-23\frac{3}{4}$, j'éstime la temperature moyenne à —13. Et consequemment je deduis $\frac{26}{1000}$, — —				4,455
Le fond du puits de la Caroline au dessous de l'éntrée du puits de la Dorothée, — — — —				170,735

3

A 6 h.

	In.	L.	16th of Lines.	Log.
At 6 o'clock at the bottom of the loweſt ſearching gallery of the Benedict, —	27	2	14 = 5230	37185,017
At the top of the pit of the Dorothy, -	26	3	15 = 5055	37037,212
Difference,				147,805
At the place of obſervation in the mine the thermometer was at $-8\frac{3}{4}$, higher by $2\frac{1}{4}$ than at the pit of the Caroline. But as this gallery communicates with the ſame pit, and that conſequently the column of air contained in it is part of that which weighs upon the air of the gallery, I am only to eſtimate the temperature of the pit. I therefore deduct $\frac{26}{1000}$, as in the preceding obſervation, —				3,843
The gallery, &c. of the Benedict, below the entrance of the Dorothy, —				143,962

Obſer-

	P.	L.	16 de Lig.	Log.
A 6 h. au fond de la galerie de recherche la plus baſſe de la Bénédicte, —	27	2	14 = 5230	37185,017
Au haut du puits de la Dorothée, —	26	3	15 = 5055	37037,212
Difference,				147,805
Au lieu de l'obſervation dans la mine, le thermométre étoit à $-8\frac{3}{4}$, plus haut de $2\frac{1}{4}$ qu'au puits de la Caroline. Mais comme cette galerie communique au même puits, et que par conſequent la colonne d'air qu'il renferme, fait partie de celle qui péſe ſur l'air de la galerie, je ne dois avoir égard qu'à la temperature du puits. Je deduis donc $\frac{26}{1000}$ comme dans l'obſervation précédente, —				3,843
La galerie, &c. de la Bénédicte, au deſſous l'entrée de la Dorothée,				143,962

Obſer-

Obfervations at the Ramelfberg.

On the 28th of October, at $9\frac{3}{4}$ h. I obferved the barometer under the cover of the entrance of the mines, two feet and a half above the aperture of the pit, and found it at — 27 2 3

At $2\frac{3}{4}$ h. when I left the mines, it was at - 27 2 8

Confequently it had rifen $\frac{5}{16}$ths of a line in five hours; whence, not having made any regular obfervations during this interval, I will fuppofe the variation to have been equable, that is, $\frac{1}{16}$th by the hour, during the time I ftayed in the mines.

At

Obfervations faites au Ramelfberg.

Le 28 Octobre, à $9\frac{3}{4}$ h. du matin, J'obfervai le Baromètre fous le couvert de l'entrée des mines, $2\frac{1}{2}$ pieds au deffus de l'ouverture du puits, et je le trouvai à — — — 27 2 3

A $2\frac{3}{4}$ h. au fortir des mines, il fe trouva à — — 27 2 8

Il avoit donc monté de $\frac{5}{16}$ de lignes en 5 heures; et n'ayant pas eu d'obfervations fuivies pendant cet intervalle, je fuppoferai la variation graduelle, et ainfi d' $\frac{1}{16}$ par heure, durant le tems que je reftai dans les mines.

A $11\frac{1}{2}$ h.

	In.	L.	16th of Lines.	Log.
At $11\frac{1}{2}$ h. at the bottom of the pit of Kaunkuhl, —	27	7	14 = 5310	37250,945
At $12\frac{1}{2}$ h. at the entrance of the gallery of Breitling 27 4 8; consequently an hour before at $11\frac{1}{2}$, -	27	4	7 = 5255	37205,729

Difference, — —		45,218
At the bottom of the pit, therm. — −5; Near the gallery, - −13	−9 mean, deduct therefore $\frac{18}{1000}$,	,813
Height of the gallery of Breitling above the bottom of the pit of Kaunkuhl, —		44,405

At

	P.	L.	16 de Lig.	Log,
A $11\frac{1}{2}$ h. au fond puits de Kaunkühl, —	27	7	14 = 5310	37250,945
A midi $\frac{1}{2}$, à l'entrée de la galerie de Breitling 27 4 8; donc 1 h. plus tòt, soit $11\frac{1}{2}$ h. —	27	4	7 = 5255	37205,727

Difference, — —		45,218
Au fond du puits, therm. —5; Auprès de la galerie, —13	—9 moyen, donc $\frac{18}{1000}$ à deduire,	,813
Hauteur de la galerie de Breitling, sur le fond du puits de Kaunkühl,		44,405

A midi

	In. L. 16th of Lines.	Log.
At $12\frac{1}{4}$ h. at the entrance of the gallery of Breitling, near the pit, — —	27 4 8 = 5226	37206,554
Under cover of the mines, at the same time, — —	27 2 6 = 5222	37178,369
Difference, — —		28,185
Therm. in the pit − 13, deduct therefore $\frac{26}{1000}$,		732
		27,453
Deduct the elevation of the barometer above the entrance of the mines, —		416
The height of the entry of the mines above the gallery of Breitling, — —		27,037

At

	P. L. 16 de Lig.	Log.
A midi $\frac{1}{4}$ à l'entrée de la galerie de Breitling, près du puits, — —	27 4 8 = 5226	37206,554
Sous le couvert des mines, à cette heure là —	27 2 6 = 5222	37178,369
Difference, — — —		28,185
Thermométre dans le puits —13; donc $\frac{26}{1000}$ à deduire, —		,732
		27,453
A deduire ce dont le baromètre étoit plus haut que l'entrée des mines,		,416
Hauteur de l'entrée des mines, sur la galerie de Breitling, —		27,037

A $2\frac{3}{4}$ h.

	In.	L.	16th of Lines.	Log.
At $2\frac{3}{4}$ h. under the cover of the mines,	27	2	8 = 5224	37180,032
At $2\frac{1}{2}$ h. at the top of the pit of Kaunkuhl, — —	26	11	6 = 5174	37138,264
Difference, — —				41,768
These two observations were made on the outside of the mines, the heat of the air being −11, deduct therefore $\frac{22}{1000}$, — —				419
				40,850
Add the elevation of the barometer under the cover, above the entrance of the mines,				918
Height of the top of the pit of Kaunkuhl above the entrance of the mines, —				41,266

At

	P.	L.	16 de Lig.	Log.
A $2\frac{3}{4}$ h. sous le couvert des mines, —	27	2	8 = 5224	37180,032
A $2\frac{1}{2}$ h. au haut du puits de Kaunkühl,	26	11	6 = 5174	37138,264
Difference; — —				41,768
Ces deux observations furent faites au dehors des mines, la chaleur de l'air étant —11; donc $\frac{22}{1000}$ à deduire, — —				,918
				40,850
A ajouter ce dont le baromètre sous le couvert, étoit au dessus de l'entrée des mines, — — — —				,416
Hauteur du haut du puits de Kaunkühl, sur l'entrée des mines,				41,266

A 11½ h.

	In.	L.	16th of Lines.	Log.
At $11\frac{1}{2}$ h. at the bottom of the pit of Kaunkuhl as above,	27	7	14=5301	37250,945
At $2\frac{1}{2}$ h. at the top of the pit 26 11 6, consequently at $11\frac{1}{2}$ h.	26	11	3=5171	37135,745
Difference,	—	—		115,200
At the bottom of the pit, therm. −5; At the top, −13	−9, consequently $\frac{18}{1000}$ to deduct,			2,074
Total depth of the pit of Kaunkuhl,	—			113,126

Obser-

	P.	L.	16 de Lig.	Log.
A $11\frac{1}{2}$ h. au fond du puits de Kaunkühl, comme ci-dessus, — —	27	7	14=5310	37250,945
A $2\frac{1}{2}$ h. au haut du puits 26 11 6, donc à $11\frac{1}{2}$ h. — —	26	11	3=5171	37135,745
Difference,	—	—		115,200
Au fond du puits, therm. —5; Au haut, —13	—9, donc $\frac{18}{1000}$ à deduire,		—	2,074
Profondeur totale du puits de Kaunkühl,	—	—		113,126

Obser-

Observations at two Points geometrically determined on the outside of the Mountain, relatively to the Mine of Alte Seegen.

	In.	L.	
On the 30th of October, at 10 in the morning, I observed the barometer under the cover of the mine called Alte Seegen near Zellerfelt, and found it at — —	21	71	2
At half an hour past seven at night, at my return from these observations, it was at	26	6	8

Consequently in nine hours and a half the barometer fell $\frac{10}{16}$ths of a line; and by observations made by Syndic LEYSER, this fall was gradual.

At

Observations en deux Points déte minés géométriquement à l'extérieur de la Montagne, relativement à la Mine d'Alte-Seegen.

	P.	L.	
Le 30 Octobre, à 10 h. du matin, j'observai le baromètre sous le couvert de la mine nommée Alte Seegen, située près de Zellerfeld, et je le trouvai à — — — —	27	71	2
A $7\frac{1}{2}$ h. du soir, au retour des observations ci après, il se trouva à	26	6	8

Ainsi en $9\frac{1}{2}$ h. le baromètre baissa de $\frac{10}{16}$ de ligne. Et par des observations faites d'heure en heure par M. le Sindic LEYSER, cet abbaissement fut graduel.

	In.	L.	16th of Lines.	Log.
At 12 h. in the valley of Bremeke, half a toise lower than a certain determined point,	27	3	2 = 5234	37188,357
At Alte Seegen, two hours after the first observation, —	27	7	0 = 5104	37079,107
Difference, — —				109,230
Heat of the air − 30, consequently $\frac{60}{1000}$ to deduct				6,554
				102,676
Deduct the half toise above mentioned, —				500
French toises, —				102,176

At

	P.	L.	16 de Lig.	Log.
A midi, dans la vallée de Bremeke, ½ toise plus bas qu'un certain point déterminé,	27	3	2 = 5234	37188,337
A Alte Seegen, 2 h. aprés la prémiere observation, — —	26	7	0 = 5104	37079,107
Difference, — —				109,230
Chaleur de l'air —30; donc $\frac{60}{1000}$ à deduire, — —				6,554
				102,676
A deduire la demi toise ci-dessus, — —				,500
Toises de France, — —				102,176

A $1\frac{3}{4}$ h.

	In.	L.	16th of Lines.	Log.
At $1\frac{3}{4}$ h. at Lasfelde without the Hartz, in the valley of Ofterode, at a determined point,	27	8	10 = 5322	37260,749
At Alte Seegen, about four hours after the firft obfervation,	27	8	10 = 5102	37077,405
				183,344
Heat of the air − 26, confequently $\frac{52}{1000}$ to deduct				9,534
French toifes, — —				173,810

Obfer-

	P.	L.	16 de Lig.	Log.
A $1\frac{3}{4}$ h. à Lasffelde, hors du Hartz, vallée d'Ofterode, à un point déterminé, —	27	8	10 = 5322	37260,749
A Alte Seegen environ 4 h. après la prèmiere obfervation, — —	26	6	14 = 5102	37077,405
				183,344
Chaleur de l'air —26; donc $\frac{52}{1000}$ à deduire, — —				9,534
Toifes de France, — —				173,810

Observations to determine the height of the Brocken *with respect to* Gottingen *and* Hanover.

	In.	L.		
The 25th of October, on setting out from Oder-brucke for the Brocken, at three quarters past eight in the morning, —	25	9	8	On the first floor in the Inn.
In returning, at $3\frac{1}{4}$ h. —	25	8	14	
Difference, —	0	0	10	

Observed at the Brocken at Noon and at 1 o'clock, the mean, at about $12\frac{1}{2}$h. makes nearly half the space of time above mentioned; consequently the height of the barometer at Oder-brucke at that hour may be reckoned at 25 9 3.

Sindic

Observations pour déterminer la hauteur du Brocken *relativement á* Gottingue *et à* Hanovre.

	P.	L.		
Le 25 Octobre, en partant d'Oder-brucke pour le Brocken, à $8\frac{3}{4}$ h. du matin, — —	25	9	8	Au prémier étage du Cabaret.
En revenant, à $3\frac{1}{4}$ h. — —	25	8	14	
Difference, — —	0	0	10	

Observé au Brocken à midi et à 1 heure. Le milieu, midi $\frac{1}{2}$, fait à peu près la moitié de l'intervalle de tems ci-dessus; et par conséquent on peut évaluér la hauteur du baromètre à Oder-brucke à cette heure la, 25 9 3.

M. le Sindic

Sindic LEYSER obſerved the barometer at his houſe at Clauſthal, in the morning and at noon; when inſtead of falling, as at Oder-brucke, it roſe half a line, and remained at that point till the evening. The only cauſe to which I can aſcribe this difference between his obſervation and mine, is the increaſe of heat in his room: but there was no obſervation with the thermometer to correct the effect of this cauſe; I ſhall therefore content myſelf with my own obſervation for this meaſure, though I employ Mr. LEYSER's for the following one.

M. le Sindic LEYSER obſerva le baromètre chez lui à Clauſthal le matin et à midi: et au lieu de baiſſer, comme à Oder-brucke, il monta de ½ ligne, et reſta à ce point juſqu'au ſoir. Je ne ſaurois attribuer cette différence entre l'obſervation de M. LEYSER et la mienne, qu'à ce que la chaleur augmenta dans ſa chambre: mais il n'y eut point d'obſervation du thermométre pour corriger l'effet de cette cauſe. Je m'en tiens donc à mon obſervation ſeule pour cette meſure-ci; quoique j'employe celle de M. LEYSER pour la ſuivante.

	In.	L.	16th of Lines.	Log.
At $12\frac{1}{2}$ h. the barometer at Oder-brucke must have been at,	25	9	3=4947	36943,419
At the Brocken, at 12 h. 23 8 5, at 1h. 5 m. 24 8 1, mean	29	8	3=4739	36756,867
Difference,		—	—	186,552

On setting out from Oder-brucke the heat of the air was at -	$-34\frac{1}{2}$	-33	$-36\frac{1}{2}$, conseq. $\frac{73}{1000}$ to deduct,	13,618
At our return, -	$-31\frac{1}{2}$			
At the Brocken, —		-40		

Height of the Brocken above Oder-brucke, 172,934

By

	P.	L.	16 de Lig.	Log.
A midi ½ le baromètre devoit être à Oder-brucke,	25	9	3=4947	36943,419
Au Brocken, à midi 24 8 5, à 1 h. 5 m. 24 8 1, moyen,	24	8	3=4739	36756,867
Difference,		—	—	186,552

En partant d'Oder-brucke, la chaleur de l'air etoit, — —	$-34\frac{1}{2}$	-33	$-36\frac{1}{2}$ donc $\frac{73}{1000}$ à deduire,	13,618
Au retour, — —	$-31\frac{1}{2}$			
Au Brocken, — — —		-40		

Hauteur du Brocken sur Oder-brucke, — — 172,934

Par

By the above mentioned obſervations of Mr. DE LEYSER's, and by the compariſon between his barometer and mine, I have reaſon to conjecture, that at the time of the mean obſervation of the barometer at Oder-brucke,

	In.	L.	16th of Lines.		Log.
it ſtood at Clauſthal at,	26	4	3	=5059	37040,647
Mean height obſerved at Oder-brucke,	25	9	3	=4947	36943,419

Difference, — — 97,228

The mean heat of the air muſt have been a little greater than at Oder-brucke; I compute it at −30, conſequently $\frac{60}{1000}$ to deduct, - 5,834

Height of Oder-brucke over the firſt floor of Mr. DE LEYSER's houſe at Clauſthal, or what comes to the ſame nearly, above the firſt-floor of the Crown Inn, where I lodged, - 91,394

The

Par les obſervations ci-deſſus de M. LEYSER, et par la comparaiſon faite de ſon barométre avec le mien, j'ai lieu de conjecturer, que lors de l'obſervation moyenne

	P.	L.	16 de Lig.		Log.
du baromètre à Oder-brucke, il étoit à Clauſthal,	26	4	3	=5059	37040,647
Hauteur moyenne obſervée à Oder-brucke,	25	9	3	=4947	36943,419

Difference, — — 97,228

La chaleur moyenne de l'air devoit être un peu plus grande qu'à Oder-brucke. Je la ſuppoſe à —30; donc $\frac{60}{1000}$ à deduire, 5,834

Hauteur d'Oder-brucke ſur le prémier étage de la maiſon de M. LEYSER à Clauſthal, ou, ce qui revient à peu près au méme, ſur le prémier étage de l'auberge de la Couronne où j'étois logé, 91,394

Le

The mean term betwixt 17 observations of the barometer, made at Gottingen from the 24th of October to the 1st of November, by Professor ERXLEBEN, corrected for the heat,

	In.	L.	16th of Lines.	Log.
rected for the heat,	27	9	3 = 5331	37268,09
The mean term of 17 observations made upon the first-floor of the Crown Inn at Clausthal, corresponding for the time to those of Gottingen,	26	4	8 = 5064	37044,94
Difference,	—		—	223,15
Mean heat of the air during the observations at Gottingen, −26½; At Clausthal, −31½	−29 conseq.		$\frac{58}{1000}$ to deduct.	12,94
Height of Clausthal above Gottingen,				210,22

Le terme moyen entre 17 observations du barométre faites à Gottingue, du 24 au 21 Octobre, par M. le Professeur ERXLEBEN, corrigées aussi pour la chaleur,

	P.	L.	16 de Lig.	Log.
chaleur, — — —	27	9	3 = 5331	37268,09
Le terme moyen de 17 observations faites au prémier étage de la Couronne à Clausthal, correspondantes pour le tems avec celles de Gottingue, — — —	26	4	8 = 5064	37044,94
Difference, — — —				223,15
Chaleur moyenne de l'air pendant les observations à Gottingue, — — — —26½; De même à Clausthal, — — —31½	—29; donc		$\frac{58}{1000}$ à déduire,	12,94
Hauteur de Clausthal sur Gottingue, — — —				210,21

Le

Mean term of 16 obſervations made at Hanover, from the 24th of October to the 1ſt of November, by Mr. DE HINUBER, with a barometer of Mr. DOLLOND's made

	In.	L.	16th of a Line.	Log.
to agree with mine,	28	2	3 = 5411	37332,775
That of the correſponding obſervations for the time, made by Mr. ERXLEBEN,	27	9	9 = 5337	37272,972
Difference, — —				59,803
Mean heat at Hanover, at the time of the obſervations, $-29\frac{1}{2}$; The ſame at Gottingen, $-26\frac{1}{2}$			-28, conſeq. $\frac{56}{1000}$ to deduct	3,349
Height of Gottingen above Hanover, —				56,454

Le terme moyen de 16 obſervations faites à Hanovre, du 24 Octobre au 1 Novembre, par M. DE HINUBER, avec un Baromètre de M. DOLLOND, mis

	P.	L.	16 de Lig.	Log.
d'accord avec le mien, — —	28	2	3 = 5411	37332,775
Celui des obſervations correſpondantes pour le tems, faites par M. ERXLEBEN, —	27	9	9 = 5337	37272,972
Difference, — —				59,803
Chaleur moyenne à Hanovre au tems des obſervations, — — — $-29\frac{1}{2}$; De même à Gottingue, — $-26\frac{1}{2}$			-28; donc $\frac{56}{1000}$ à déduire,	3,349
Hauteur de Gottingue ſur Hanovre, — —				56,454

XXIII. *The general Mathematical Laws which regulate and extend Proportion universally; or, a Method of comparing Magnitudes of any Kind together, in all the possible Degrees of Increase and Decrease. By* James Glenie, *A. M. and Lieutenant in the Royal Regiment of Artillery.*

Read March 6, 1777.

THE doctrine of proportion laid down by EUCLID, and the application of it given by him in his Elements, form the basis of almost all the geometrical reasoning made use of by mathematicians both ancient and modern. But the reasonings of geometers with regard to proportional magnitudes have seldom been carried beyond the triplicate *ratio*, which is the proportion that similar solids have to one another when referred to their homologous linear dimensions. This boundary, however, comprehends but a very limited portion of universal comparison, and almost vanishes into nothing when referred to that endless variety of relations, which must necessarily take place between geometrical magnitudes, in the infinite possible degrees of increase and decrease. The first of these takes in but

a very

a very contracted field of geometrical comparifon; whereas the laft extends it indefinitely. Within the narrow compafs of the firft, the ancient geometers performed wonders, and their labours have been pufhed ftill farther by the ingenuity and indefatigable induftry of the moderns. But no author, that I have been able to meet with, gives the leaft hint or information with regard to any general method of expreffing geometrically, when any two magnitudes of the fame kind are given, what degree of augmentation or diminution any one of thefe magnitudes muft undergo, in order to have to the other any multiplicate or fub-multiplicate *ratio* of thefe magnitudes in their given ftate; or any fuch *ratio* of them as is denoted by fractions or furds; or (to fpeak ftill more generally) a *ratio* which has, to the *ratio* of the firft-mentioned of thefe magnitudes to the other, the *ratio* of any two magnitudes whatever of the fame but of any kind. Neither have I been able to find that any author has fhewn geometrically in a general way, when any number of *ratios* are to be compounded or decompounded with a given *ratio*, how much either of the magnitudes in the given *ratio* is to be augmented or diminifhed, in order to have to the other a *ratio*, which is equal to the given *ratio*, compounded or decompounded with the other *ratios*. To inveftigate all thefe geome-

 trically,

pendicularly to VO, or otherwife if in the fame angle; and let the rectangles or parallelograms MR, NP, be completed. Let LM be a fourth proportional to OP, MN and NR−OP; and let the rectangle or parallelogram LQ be completed.

Then (14. E. 6.) LT is equal to TR, and confequently LQ to MR. But (23. E. 6.) MR has to NP the *ratio* compounded of the *ratios* of MN to NO and NR to OP. Therefore (1. E. 6.) LN has to NO the *ratio* compounded of the *ratios* of MN to NO and NR to OP. But LN is equal to $MN + MN.\frac{NR-OP}{OP}$, or $A + A.\frac{C-D}{D}$, by conftruction. Whence it appears, that a magnitude of the fame kind with A and B, which has to B the *ratio* compounded of the *ratios* of A to B and C to D, is expreffed by $A + A.\frac{C-D}{D}$.

In like manner let E, F, be reprefented by RN, OP, refpectively, and let LK be a fourth proportional to OP, LN, and QR. Then (14. E. 6.) KX is equal to XR or TR and XS together. But fince LN hath already been fhewn to be equal to $A + A.\frac{C-D}{D}$, LK is a fourth proportional to F, E−F, and $A + A.\frac{C-D}{D}$; that is equal to $A.\frac{E-F}{F} + A.\frac{C-D}{D}.\frac{E-F}{F}$. by conftruction. Wherefore KN being equal to LK+LN is equal to $A + A.\frac{C-D}{D} + A.\frac{E-F}{F} + A.\frac{C-D}{D}.\frac{E-F}{F}$. And fince KQ is equal to LR, KN has to NO a *ratio* compounded of

 the

the *ratios* of LN to NO and NR to OP; that is, of the *ratios* A to B, C to D, and E to F. Therefore a magnitude of the ſame kind with A and B, which has to B the *ratio* compounded of theſe *ratios* is expreſſed by $A + A.\frac{C-D}{D} + A.\frac{E-F}{F} + A.\frac{C-D}{D}.\frac{E-F}{F}$.

Again, if NR, OP, be ſuppoſed to repreſent G, H, reſpectively, and KV a fourth proportional to OP, KN, and QR; VQ will be equal to KR (14. E. 6.) and conſequently VN will have to NO a *ratio* compounded of the *ratios* of KN to NO and NR to OP; that is, of the *ratios* A to B, C to D, E to F, G to H. But VK is by conſtruction equal to $A.\frac{G-H}{H} + A.\frac{C-D}{D}.\frac{G-H}{H} + A.\frac{E-F}{F}.\frac{G-H}{H} + A.\frac{C-D}{D}.\frac{E-F}{F}.\frac{G-H}{H}$. And this added to KN above found gives $A + A.\frac{C-D}{D} + A.\frac{E-F}{F} + A.\frac{G-H}{H} + A.\frac{C-D}{D}.\frac{E-F}{F} + A.\frac{C-D}{D}.\frac{G-H}{H} + A.\frac{E-F}{F}.\frac{G-H}{H} + A.\frac{C-D}{D}.\frac{E-F}{F}.\frac{G-H}{H}$, for the magnitude of the ſame kind with A and B, which has to B the *ratio* compounded of the *ratios* A to B, C to D, E to F, G to H; whence the law of continuation is manifeſt.

The ſame concluſions may be derived from (E. 5.); ſo that no principle can be ſimpler or more geometrical than that here made uſe of.

Thus then theſe magnitudes will ſtand.

1. $A + A.\frac{C-D}{D}$, when two *ratios* are compounded.

2. $A+A.\frac{C-D}{D}+A.\frac{E-F}{F}+A.\frac{C-D}{D}.\frac{E-F}{F}$, when three are compounded.

3. $A+A.\frac{C-D}{D}+A.\frac{E-F}{F}+A.\frac{G-H}{H}+A.\frac{C-D}{D}.\frac{G-F}{F}+A.\frac{C-D}{D}.\frac{G-H}{H}+A.\frac{E-F}{F}.\frac{G-H}{H}+A.\frac{C-D}{D}.\frac{E-F}{F}.\frac{G-H}{H}$, when four *ratios* are compounded, &c. &c.

By continuing this operation much farther, I found upon examination that the number of terms in which A is connected with the differences C−D, E−F, G−H, &c. taken one by one, two by two, three by three, &c. if p denote the number of *ratios* compounded, is expressed respectively by $\frac{p-1}{1}$, $\frac{p-1}{1}.\frac{p-2}{2}$, $\frac{p-1}{1}.\frac{p-2}{2}$, $\frac{p-3}{3}$, &c. Thus if the *ratio* of A to B be supposed equal to the *ratios* of C to D, E to F, G to H, &c. respectively, these expressions will give the following ones.

1. $A+\frac{2-1}{1}.A.\frac{A-B}{B}$.

2. $A+\frac{3-1}{1}.A.\frac{A-B}{B}+\frac{3-1}{1}.\frac{3-2}{2}.A.\frac{\overline{A-B}|^2}{B}$.

3. $A+\frac{4-1}{1}.A.\frac{A-B}{B}+\frac{4-1}{1}.\frac{4-2}{2}.A.\frac{\overline{A-B}|^2}{B}+\frac{4-1}{1}.\frac{4-2}{2}.\frac{4-3}{3}.A.\frac{\overline{A-B}|^3}{B}$; for magnitudes of the same kind with A and B, which have to B respectively the duplicate, triplicate, and quadruplicate *ratio* of A to B; where p is successively equal to 2, 3, and 4. And universally, by the same geometrical reasoning, it is found, that $A+\frac{p-1}{1}.A.\frac{A-B}{B}+\frac{p-1}{1}.\frac{p-2}{2}.A.\frac{\overline{A-B}|^2}{B}+$ &c. $A.\frac{\overline{A-B}|^{p-1}}{B}$ has to

B such

B ſuch a multiplicate *ratio* of A to B as is expreſſed by the number p.

In the reaſoning above I fixed on B as the magnitude to which the reſt were to be referred; but I might as well have fixed on A or any of the other magnitudes. Thus, for inſtance, $B + \frac{p-1}{1} . B . \frac{B-A}{A} + \frac{p-1}{1} . \frac{p-2}{2} . B . \frac{\overline{B-A}|^{2}}{A} + \&c.$ $B . \frac{\overline{B-A}|^{p-1}}{A}$ has to A ſuch a multiplicate *ratio* of B to A as is expreſſed by the number p; or A has to $B + \frac{p-1}{1} . B . \frac{B-A}{A} + \frac{p-1}{1} . \frac{p-2}{2} . B . \frac{\overline{B-A}|^{2}}{A} + \&c.$ $B . \frac{\overline{B-A}|^{p-1}}{A}$ the *ratio* of $A + \frac{p-1}{1} . A . \frac{A-B}{B} + \frac{p-1}{1} . \frac{p-2}{2} . A . \frac{\overline{A-B}|^{2}}{B} + \&c.$ $A . \frac{\overline{A-B}|^{p-1}}{B}$ to B; that is, ſuch a multiplicate *ratio* of A to B as is expreſſed by the number p. Each of theſe, indeed, I demonſtrated ſeparately from the ſame ſort of geometrical reaſoning; but for the ſake of brevity I omit ſetting down theſe ſeparate demonſtrations, as they are both contained in general reaſoning above, which furniſhes likewiſe a great variety of other expreſſions, according as certain numbers of the *ratios* C to D, E to F, G to H, &c. are ſuppoſed to be reſpectively equal to, greater or leſs than, the *ratio* of A to B.

XXIV. *The Case of* Ann Davenport. *By Mr.* Fielding Best Fynney, *Surgeon at* Leek, *in* Staffordshire; *communicated to* Thomas Percival, *M. D. F. R. S. and by him to Sir* John Pringle, *Bart. P. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

Manchester, April 8, 1777.

SIR,

Read April 10, 1777.

I TOOK the liberty, a few days ago, of transmitting to you, by a private hand, a singular case, sent me by Mr. F. B. FYNNEY, an ingenious surgeon, who is settled at Leek in Staffordshire. He desires that it may be laid before the Royal Society; and I shall think myself much obliged by your compliance with his request.

TO DR. PERCIVAL.

SIR, Leek, March 10, 1777.

UPON May 16, 1775, being defired to vifit ANN DAVENPORT, a native of this town, I beheld a truly miferable object, with the moft cadaverous countenance I had ever feen, emaciated to the laft degree by a hectic fever, and profufe colliquative fweats. She had a continual thirft, her appetite was totally gone, and fhe was always in the extremes of being too loofe or too bound.

Her mother informed me, that fhe was then in her twenty-firft year; and that fhe had been a ftrong and fprightly child from her birth, until fhe was about five years of age, from which time fhe had been a ftranger to health, and every now and then had been feized with excruciating fits of the colic, efpecially whenever fhe ate or drank any thing the leaft acid.

The young woman told me, that about a year ago fhe had firft perceived a fwelling on the right fide of her belly, juft above the groin; which, if at any time fhe attempted to ftretch out her thigh, gave her inexpreffible pain, as if fomething ftabbed her in that part:

 that

that therefore ſhe was always obliged to keep up her knees, more or leſs, towards her breaſt, by which means ſhe had, in ſome degree, loſt the power of extending her limbs.

I ordered her to take half a drachm of powdered bark in a little red port wine every four hours; and, as matter had already formed within the tumor, I deſired that a maturating poultice might be applied every night and morning; for I imagined that nature, without ſuch aſſiſtance, could never bring the abſceſs to a head in her weak condition.

July 10th, the matter pointing at the upper end of the tumour very near the *os ilium*, I made a large opening, from which was diſcharged an amazing quantity of *pus*; but, as the tenſion was ſtill great, I applied a linſeed poultice over the common dreſſings: nevertheleſs, in a few days a ſecond abſceſs began to form towards the *vertebræ* of the loins, between the falſe ribs and the *os ilium*, which was rapid in its progreſs, for it was brought to maturation, and opened on the 26th.

On the 31ſt I was alarmed with a gangrenous appearance of the whole integuments of the *abdomen*: for this ſhe took one drachm of powdered bark in red-port every three hours; but, as veſications and every ſym-

ptom of a *sphacelus* continued to increase, I likewise used the bark externally, in the two following forms, every morning and evening:

℞ *Tinct. Cort. Peruv. Simp.* ℥ ij
—— *Myrrh. Comp.* ℥ j
Sp. Sal. Ammon.
Mell. Ægypt. aa. ℥ ss. *m. fiat embrocat.*

℞ *Fæc. Cerevis. acid.* ℔ j
Farin. Avenac. q. s. coque ad consistentiam Cataplasmatis, et adde Pulv. Cort. Peruv. ℥ j
Ol. Olivar. rec. ℥ iv *m. fiat Cataplasm.*

This treatment soon put the mortification to a stand, and the parts sloughing off largely left three holes, at nearly equal distances one from another, betwixt the first opening and the left *os ilium*, besides several ones in different parts of the belly; but as the discharge was immoderate, I looked upon the patient to be in the utmost danger. However, the same course was persevered in, and at the latter end of August another abscess appeared lower down, towards the right groin; I ordered it to be poulticed, and left it to open of itself, which it did on the 21st of September. I was immediately called to her; and,

and, upon carefully examining the part, I found a hard ſubſtance deeply ſeated, which I directly extracted[(a)].

It was making its way towards the integuments from the extremity of the *appendix vermiformis* of the *cæcum*, which probably, and fortunately, by former inflammations had adhered to the *peritonæum*. The large end came firſt, and the ſmall end was within the *appendix vermiformis* of the *cæcum* at the time I took it out; for, immediately upon the extraction, ſome excrements followed, and among them ſome dark brown particles which I diſcovered to be filings of iron, which the patient had formerly taken in a large quantity, as ſhe had never been regular like other women. On a careful examination I found ſome of theſe filings quite reduced to ruſt,

(a) See plate IX. the figures 1. and 2. are different views of the external ſurface of this irregular ſubſtance, and of ſo much of its *nucleus* as projects out of the round part, exactly as both appeared on being taken out of the body. The whole was of a duſky brown colour, and had a great reſemblance to a ſmall ſhrivelled pear. Fig. 3. is a ſection of the round part, which ſeemed to be formed of fine fibrous ſubſtances, cloſely cemented together by an earthy matter, and of the peg of crab-tree wood, its *nucleus*. This figure likewiſe ſhews how far the peg went in, and alſo an incruſtation of ſtony matter upon it.

The *nucleus*, I believe, is the ſmaller end of that part of a ſilk-engine called a ſtar, at which machine the patient had been employed before ſhe was five years of age, therefore it muſt have been lodged at leaſt fixteen years within the *appendix vermiformis* of the *cæcum*, as ſhe remembers nothing of ſwallowing it, and as during that courſe of years ſhe had frequently been afflicted with the ſevere colics before-mentioned.

but

but ftill retaining their form as they came from under the file.

Some *fæces* came through this laft wound daily, frequently moft copioufly; and fometimes (though the external orifice was large) by being confined with the dreffings, they infinuated themfelves between the integuments of the *abdomen*, and came through the other openings. About the middle of February 1776, the difcharge of the excrements by thefe openings was fenfibly diminifhed; and the wounds were all healed, except one, by the latter end of the year, through which a fmall quantity of excrements ftill continue to pafs now and then.

Her health is, within this fhort time, furprizingly improved; fhe is now very flefhy and ftrong, has had the *catamenia*, and I have the greateft reafon to expect that fhe will be perfctly cured. Strict regard was all along paid to the non-naturals.

As the cafe above is a very uncommon one, I prevailed on the reverend Mr. ROGERS, minifter of this place, to vifit the patient along with me; and he was fo fully fatisfied of the truth of the account, as to give me the following certificate.

I hereby

I hereby certify, that I ſaw, this 27th day of December, 1775, Mr. FIELDING BEST FYNNEY dreſs his patient ANN DAVENPORT, and was an eye-witneſs to pure *fæces* coming out of the wound from which he had extracted an irregular ſubſtance on the 21ſt of September laſt.

As witneſs my hand,

J. ROGERS,
Miniſter of Leek.

Fig 1.

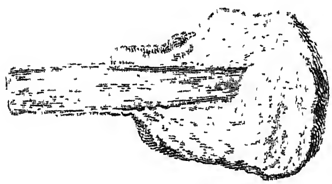


Fig 3.

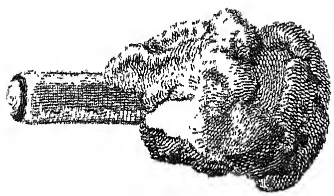


Fig. 2.

J. Basire del et sculp.

XXV. *An Account of the Kingdom of* Thibet. *In a Letter from* John Stewart, *Esquire, F. R. S. to Sir* John Pringle, *Bart. P. R. S.*

S I R,

London,
March 20, 1777.

Read April 17, 1777.

DURING my late reſidence in India, a tranſaction took place in Bengal, which, in its conſequence, led to a new and more intimate knowledge of a vaſt country, hitherto unexplored by Europeans, and hardly known to them but by name. As every diſcovery of this ſort tends to the advancement of natural knowledge, I have thought a ſhort notice on the ſubject might prove no diſagreeable communication to the Society; and therefore take the liberty, with your approbation, to ſubmit it, in this manner, to them.

The kingdom of Thibet, although known by name ever ſince the days of MARCO PAOLO and other travellers of the twelfth and thirteenth centuries, had never been properly explored by any European till the period of

which I am now to ſpeak. It is true, ſome ſtraggling miſſionaries of the begging orders had, at different times, penetrated into different parts of the country; but their obſervations, directed by ignorance and ſuperſtition, placed in a narrow ſphere, could give no ideas but what were falſe and imperfect. Since them, the Jeſuits have given the world, in DUHALDE'S Hiſtory of China, a ſhort account of this country, collected, with their uſual pains and judgement, from Tartar relations, which, as far as it goes, ſeems to be pretty juſt.

This country commonly paſſes in Bengal under the name of Boutan. It lies to the northward of Hindoſtan, and is all along ſeparated from it by a range of high and ſteep mountains, properly a continuation of the great Caucaſus, which ſtretches from the ancient Media and the ſhores of the Caſpian ſea, round the north-eaſt frontiers of Perſia, to Candahar and Caſſamire, and thence, continuing its courſe more eaſterly, forms the great northern barrier to the various provinces of the Mogol empire, and ends, as we have reaſon to believe, in Aſſam or China. This ſtupendous Tartar bulwark had ever been held impaſſable by the Mogols, and all other Muſſulman conquerors of India: and although in the vallies lying between the lower mountains, which run out perpendicular to the main ridge, there reſide various Indian people,

whom they had occaſionally made tributary to their power, they never had attempted a ſolid or permanent dominion over them. It was on occaſion of a diſputed ſucceſſion between the heirs of one of the Rajah's or petty ſovereigns of thoſe people, that the Boutaners were called down from their mountains to the aſſiſtance of one of the parties; and our government engaged on the oppoſite ſide. The party aſſiſted by us did not fail in the end to prevail; and in the courſe of this little war two people became acquainted who, although near neighbours, were equally ſtrangers to each other. At the attack of a town called Cooch Behar, our troops and the Boutaners firſt met; and nothing could exceed their mutual ſurprize in the rencounter. The Boutaners, who had never met in the plains any other than the timid Hindoos flying naked before them, ſaw, for the firſt time, a body of men, uniformly cloathed and accoutred, moving in regular order, and led on by men of complexion, dreſs, and features, ſuch as they had never beheld before: and then the management of the artillery, and inceſſant fire of the muſquetry, was beyond any idea which they could have conceived of it. On the other hand, our people found themſelves on a ſudden engaged with a race of men unlike all their former opponents in India, uncouth in their appearance, and fierce in their aſſault, wrapped up in furs,

and armed with bows and arrows and other weapons peculiar to them.

The place was carried by our troops, and a great many things taken in the ſpoil, ſuch as arms, cloathing, and utenſils of various ſorts. Images in clay, in gold, in ſilver, and in enamel, were ſent down to Calcutta; all which appeared perfectly Tartar, as we have them repreſented in the relations and drawings of travellers; and there were beſides ſeveral pieces of Chineſe paintings and manufactures. Whilſt thoſe things continued to be the ſubject of much converſation and curioſity to us in Bengal, the fame of our exploits in the war had reached the court of Thibet, and awakened the attention of the Tayſhoo Lama, who (the Delai Lama being a minor) was then at the head of the ſtate. The Dah Terriah, or Deb Rajah as he is called in Bengal (who rules immediately over the Boutaners, and had engaged them in the war) being a feudatory of Thibet, the Lama thought it proper to interpoſe his good offices, and in conſequence ſent a perſon of rank to Bengal, with a letter and preſents to the governor, to ſolicit a peace for the Dah, as his vaſſal and dependant.

Mr. HASTINGS, the governor, did not heſitate a moment to grant a peace at the mediation of the Lama, on the moſt moderate and equitable terms; and, eager to

ſeize

ſeize every opportunity which could promote the intereſt and glory of this nation, and tend to the advancement of natural knowledge, propoſed in council to ſend a perſon in a public character to the court of the Tayſhoo Lama, to negotiate a treaty of commerce between the two nations, and to explore a country and people hitherto ſo little known to Europeans. Mr. BOGLE, an approved ſervant of the company, whoſe abilities and temper rendered him every way qualified for ſo hazardous and uncommon a miſſion, was pitched on for it. It would be foreign to my purpoſe to enter into a detail of his progreſs and ſucceſs in this buſineſs: it will be ſufficient to ſay, that he penetrated, acroſs many difficulties, to the center of Thibet; reſided ſeveral months at the court of the Tayſhoo Lama; and returned to Calcutta, after an abſence of fifteen months on the whole, having executed his commiſſion to the entire ſatisfaction of the adminiſtration. I have reaſon to believe that Mr. BOGLE will one day give to the world a relation of his journey thither, accompanied with obſervations on the natural and political ſtate of the country. I only, in the mean time, beg leave to mention a few particulars, ſuch as my recollection of his letters and papers enable me to give.

Mr. BOGLE divides the territories of the Delai Lama into two different parts. That which lyes immediately contiguous

contiguous to Bengal, and which is called by the inhabitants Docpo, he diſtinguiſhes by the name of Boutan; and the other, which extends to the northward as far as the frontiers of Tartary, called by the natives Pû, he ſtyles Thibet. Boutan is ruled by the Dah Terriah or Deb Rajah, as I have already remarked. It is a country of ſteep and inacceſſible mountains, whoſe ſummits are crowned with eternal ſnow; they are interſected with deep vallies, through which pour numberleſs torrents that increaſe in their courſe, and at laſt, gaining the plains, loſe themſelves in the great rivers of Bengal. Theſe mountains are covered down their ſides with foreſts of ſtately trees of various ſorts; ſome (ſuch as pines, &c) which are known in Europe; others, ſuch as are peculiar to the country and climate. The vallies and ſides of the hills which admit of cultivation are not unfruitful, but produce crops of wheat, barley, and rice. The inhabitants are a ſtout and warlike people, of a copper complexion, in ſize rather above the middle European ſtature, haſty and quarrelſome in ther temper, and addicted to the uſe of ſpirituous liquors; but honeſt in their dealings, robbery by violence being almoſt unknown among them. The chief city is Taſſey Seddein ſituated on the Patchoo. Thibet begins properly from the top of the great ridge of the Caucaſus, and extends from thence

in

in breadth to the confines of Great Tartary, and perhaps to ſome of the dominions of the Ruſſian empire. Mr. BOGLE ſays, that having once attained the ſummit of the Boutan mountains, you do not deſcend in an equal proportion on the ſide of Thibet; but, continuing ſtill on a very elevated baſe, you traverſe vallies which are wider and not ſo deep as the former, and mountains that are neither ſo ſteep, nor apparently ſo high. On the other hand, he repreſents it as the moſt bare and deſolate country he ever ſaw. The woods, which every where cover the mountains in Boutan, are here totally unknown; and, except a few ſtraggling trees near the villages, nothing of the ſort to be ſeen. The climate is extremely ſevere and rude. At Chamnànning, where he wintered, although it be in latitude 31° 39′, only 8° to the northward of Calcutta, he often found the thermometer in his room at 29° under the freezing point by FAHRENHEIT's ſcale; and in the middle of April the ſtanding waters were all frozen, and heavy ſhowers of ſnow perpetually fell. This, no doubt, muſt be owing to the great elevation of the country, and to the vaſt frozen ſpace over which the north wind blows uninterruptedly from the pole, through the vaſt deſarts of Siberia and Tartary, till it is ſtopped by this formidable wall.

The Thibetians are of a ſmaller ſize than their ſouthern neighbours, and of a leſs robuſt make. Their complexions are alſo fairer, and many of them have even a ruddineſs in their countenances unknown in the other climates of the eaſt. Thoſe whom I ſaw at Calcutta appeared to have quite the Tartar face. They are of a mild and chearful temper; and Mr. BOGLE ſays, that the higher ranks are polite and entertaining in converſation, in which they never mix either ſtrained compliments or flattery. The common people, both in Boutan and Thibet, are cloathed in coarſe woollen ſtuffs of their own manufacture, lined with ſuch ſkins as they can procure; but the better orders of men are dreſſed in European cloth, or China ſilk, lined with the fineſt Siberian furs. The ambaſſador from the Deb Rajah, in his ſummer-dreſs at Calcutta, appeared exactly like the figures we ſee in the Chineſe paintings, with the conical hat, the tunick of brocaded ſilk, and light boots. The Thebetian who brought the firſt letter from the Lama was wrapped up from head to foot in furs. The uſe of linen is totally unknown among them. The chief food of the inhabitants is the milk of their cattle, prepared into cheeſe, butter, or mixed with the flour of a coarſe barley or of peas, the only grain which their ſoil produces; and even theſe articles are in a ſcanty proportion: but they are furniſhed

with

with rice and wheat from Bengal and other countries in their neighbourhood. They alſo are ſupplied with fiſh from the rivers in their own and the neighbouring provinces, ſalted and ſent into the interior parts. They have no want of animal food from the cattle, ſheep, and hogs, which are raiſed on their hills; and are not deſtitute of game, though I believe it is not abundant. They have a ſingular method of preparing their mutton, by expoſing the carcaſe entire, after the bowels are taken out, to the Sun and bleak northern winds which blow in the months of Auguſt and September, without froſt, and ſo dry up the juices and parch the ſkin, that the meat will keep uncorrupted for the year round. This they generally eat raw, without any other preparation. Mr. BOGLE was often regaled with this diſh, which, however unpalatable at firſt, he ſays, he afterwards preferred to their dreſſed mutton juſt killed, which was generally lean, tough, and rank. It was alſo very common for the head men, in the villages through which he paſſed, to make him preſents of ſheep ſo prepared, ſet before him on their legs as if they had been alive, which at firſt had a very odd appearance.

The religion and political conſtitution of this country, which are intimately blended together, would make a conſiderable chapter in its hiſtory. It ſuffices for me to

 ſay,

ſay, that at preſent, and ever ſince the expulſion of the Eluth Tartars, the kingdom of Thibet is regarded as depending on the empire of China, which they call Cathay; and there actually reſide two mandarines, with a garriſon of a thouſand Chineſe, at Lahaſſa the capital, to ſupport the government; but their power does not extend far: and in fact the Lama, whoſe empire is founded on the ſureſt grounds, perſonal affection and religious reverence, governs every thing internally with unbounded authority. Every body knows that the Delai Lama is the great object of adoration for the various tribes of heathen Tartars, who roam through the vaſt tract of continent which ſtretches from the banks of the Volga to Correa on the ſea of Japan, the moſt extenſive religious dominion, perhaps, on the face of the globe. He is not only the ſovereign Pontiff, the vicegerent of the Deity on earth; but, as ſuperſtition is ever the ſtrongeſt where it is moſt removed from its object, the more remote Tartars abſolutely regard him as the Deity himſelf. They believe him immortal, and endowed with all knowledge and virtue. Every year they come up from different parts, to worſhip and make rich offerings at his ſhrine; even the emperor of China, who is a Mantchou Tartar, does not fail in acknowledgements to him in his religious capacity, and actually entertains at a great

expence,

expence, in the palace of Pekin, an inferior Lama, deputed as his Nuncio from Thibet. It is even reported, that many of the Tartar chiefs receive certain prefents, confifting of fmall portions of that, from him, which is ever regarded in all other perfons as the moft humiliating proof of human nature, and of being fubject to its laws, and treafure it up with great reverence in gold boxes, to be mixed occafionally in their ragouts. It is, however, but juftice to declare, that Mr. BOGLE ftrenuoufly infifts, that the Lama never makes fuch prefents; but that he often diftributes little balls of confecrated flour, like the *pain benït* of the Roman catholics, which the fuperftition and blind credulitity of his Tartar votaries may afterwards convert into what they pleafe. The orthodox opinion is, that when the grand Lama feems to die, either of old age or of infirmity, his foul in fact only quits an actual crazy habitation to look for another younger or better, and it is difcovered again in the body of fome child, by certain tokens known only to the Lamas or Priefts, in which order he always appears. The prefent Delai Lama is an infant, and was difcovered only a few years ago by the Tayfhoo Lama, who in authority and fanctity of character is next to him, and confequently, during the other's minority, acts as chief. The Lamas, who form the moft numerous as well as the moft

 powerful

powerful body in the ſtate, have the prieſthood entirely in their hands; and, beſides, fill up many monaſtic orders which are held in great veneration among them. Celibacy, I believe, is not poſitively enjoined to the Lamas; but it is held indiſpenſable for both men and women, who embrace a religious life: and indeed their celibacy, their living in communities, their cloyſters, their ſervice in the choirs, their ſtrings of beads, their faſts, and their penances, give them ſo much the air of Chriſtian monks, that it is not ſurprizing an illiterate capuchin ſhould be ready to hail them brothers, and think he can trace the features of St. Francis in every thing about them. It is an old notion, that the religion of Thibet is a corrupted Chriſtianity; and even Father DISEDERII, a Jeſuit (but not of the Chineſe miſſion) who viſited the country about the beginning of this century, thinks he can reſolve all their myſteries into ours; and aſſerts, with a true myſtical penetration, that they have certainly a good notion of the Trinity, ſince, in their addreſs to the Deity, they ſay as often Konciok-oik in the plural as Konciok in the ſingular, and with their roſaries pronounce theſe words, Om, ha, hum. The truth is, that the religion of Thibet, from whence-ever it ſprung, is pure and ſimple in its ſource, conveying very exalted notions of the Deity, with no contemptible ſyſtem of morality; but in its

progreſs

progreſs it has been greatly altered and corrupted by the inventions of worldly men, a fate we can hardly regret in a ſyſtem of error, ſince we know that that of truth has been ſubject to the ſame. Polygamy, at leaſt in the ſenſe we commonly receive the word, is not in practice among them; but it exiſts in a manner ſtill more repugnant to European ideas; I mean in the plurality of huſbands, which is firmly eſtabliſhed and highly reſpected there. In a country where the means of ſubſiſting a family are not eaſily found, it ſeems not impolitic to allow a ſet of brothers to agree in raiſing one, which is to be maintained by their joint efforts. In ſhort, it is uſual in Thibet for the brothers in the family to have a wife in common, and they generally live in great harmony and comfort with her; not but ſometimes little diſſenſions will ariſe (as may happen in families conſtituted upon different principles) an inſtance of which Mr. BOGLE mentions in the caſe of a modeſt and virtuous lady, the wife of half a dozen of the Tayſhoo Lama's nephews, who complained to the uncle, that the two youngeſt of her huſbands did not furniſh that ſhare of love and benevolence to the common ſtock which duty and religion required of them. In ſhort, however ſtrange this cuſtom may appear to us, it is an undoubted fact that it prevails in Thibet in the manner I have deſcribed.

The manner of beſtowing their dead is alſo ſingular: they neither put them in the ground like the Europeans, nor burn them like the Hindoos; but expoſe them on the bleak pinnacle of ſome neighbouring mountain, to be devoured by wild beaſts and birds of prey, or waſted away by time and the viciſſitudes of weather in which they lie. The mangled carcaſes and bleached bones lye ſcattered about; and, amidſt this ſcene of horror, ſome miſerable old wretch, man or woman, loſt to all feelings but thoſe of ſuperſtition, generally ſets up an abode, to perform the diſmal office of receiving the bodies, aſſigning each a place, and gathering up the remains when too widely diſperſed.

The religion of Thibet, although it be in many of its principal *dogmata* totally repugnant to that of the Bramins or of India, yet in others it has a great affinity to it. They have, for inſtance, a great veneration for the cow; but they transfer it wholly from the common ſpecies to that which bears the tails, of which I ſhall ſpeak hereafter. They alſo highly reſpect the waters of the Ganges, the ſource of which they believe to be in Heaven; and one of the firſt effects which the treaty with the Lama produced, was an application to the governor-general, for leave to build a place of worſhip on its banks. This it may be imagined was not refuſed; and,

when I left Bengal, a ſpot of ground was actually aſ-ſigned for that purpoſe, about two or three miles from Calcutta. On the other hand, the Sunniaſſes, or Indian pilgrims, often viſit Thibet as a holy place, and the Lama always entertains a body of two or three hundred in his pay. The reſidence of the Delai Lama is at Pateli, a vaſt palace on a mountain near the banks of the Barampoo-ter, about ſeven miles from Lahaſſa. The Tayſhoo Lama has ſeveral palaces or caſtles, in one of which Mr. BOGLE lived with him five months. He repreſents the Lama as one of the moſt amiable as well as intelligent men he ever knew; maintaining his rank with the ut-moſt mildneſs of authority, and living in the greateſt pu-rity of manners, without ſtarchneſs or affectation. Every thing within the gates breathed peace, order, and digni-fied elegance. The caſtle is of ſtone or brick, with many courts, lofty halls, terraces, and porticos; and the apart-ments are in general roomy, and highly finiſhed in the Chineſe ſtile, with gilding, painting, and varniſh. There are two conveniencies to which they are utter ſtrangers, ſtair-caſes and windows. There is no acceſs to the upper rooms but by a ſort of ladders of wood or iron; and for windows they have only holes in the cielings, with pent-houſe covers, contrived ſo as to ſhut up on the weather-ſide. Firing is ſo ſcarce, that little is uſed but for culinary

purpoſes;

purpoſes; and they truſt altogether for warmth in their houſes to their furs and other cloathing. The Lama, who is compleatly converſant in what regards Tartary, China, and all the kingdoms in the Eaſt, was exceedingly inquiſitive about Europe, its politics, laws, arts and ſciences, government, commerce, and military ſtrength; on all which heads Mr. BOGLE endeavoured to ſatisfy him, and actually compiled for his ſervice a brief ſtate of Europe in the Hindoſtan language, which he ordered to be tranſlated into that of Thibet. The Lama being born at Latack, a frontier province next Caſſamire, is fully maſter of the Hindoſtan language, and always converſed with Mr. BOGLE in it; but the people, who are perſuaded he underſtands all languages, believed he ſpoke to him in Engliſh, or, as they call it, the European tongue. The Ruſſian Empire was the only one in Europe known to him: he has a high idea of its riches and ſtrength, and had heard of its wars and ſucceſs againſt the Empire of Rome (for ſo they call the Turkiſh ſtate); but could not conceive it could be in any wiſe a match for Cathay. Many of the Tartar ſubjects of Ruſſia come to Thibet; and the Czar has even, at various times, ſent letters and preſents to the Lama. Mr. BOGLE ſaw many European articles in his hands; pictures, looking-glaſſes, and trinkets of gold, ſilver, and ſteel, chiefly Engliſh, which

he

he had received that way, particularly a GRAHAM's repeating watch, which had been dead, as they ſaid, for ſome time. While he was there, ſeveral Mongols and Calmucs arrived from Siberia, with whom he converſed.

The city of Lahaſſa, which is the capital, is of no inconſiderable ſize, and is repreſented as populous and flouriſhing. It is the reſidence of the chief officers of government, and of the Chineſe mandarins and their ſuite. It is alſo inhabited by Chineſe and Caſſemirian merchants and artificers, and is the daily reſort of numberleſs traders from all quarters, who come in occaſional parties, or in ſtated caravans. The waters of the Great River, as it is emphatically called in their language, waſh its walls. Father DUHALDE, with great accuracy, traces this river, which he never ſuſpects to be the Barampooter, from its origin in the Caſſemirian mountains (probably from the ſame ſpring which gives riſe to the Ganges) through the great valley of Thibet, till, turning ſuddenly to the ſouthward, he loſes it in the kingdom of Aſſam; but ſtill, with great judgement and probability of conjecture, ſuppoſes it reaches the Indian ſea ſomewhere in Pegu or Aracan. The truth is, however, that it turns ſuddenly again in the middle of Aſſam, and, traverſing that country weſterly, enters Bengal towards Rangamatty, under the above-mentioned name, and thence

bending its courſe more ſoutherly, joins the Ganges, its ſiſter and rival, with an equal, if not more copious ſtream; forming at the conflux a body of running freſh water, hardly to be paralleled in the known world, which diſembogues itſelf into the Bay of Bengal. Two ſuch rivers uniting in this happy country, with all the beauty, fertility, and convenience which they bring, well entitles it to the name of the Paradiſe of Nations, always beſtowed upon it by the Moguls.

The chief trade from Lahaſſa to Pekin is carried on by caravans that employ full two years in the journey thither and back again; which is not ſurprizing, when we conſider that the diſtance cannot be leſs than two thouſand Engliſh miles; and yet it is to be obſerved, that an expreſs from Lahaſſa reaches Pekin in three weeks, a circumſtance much to the honour of the Chineſe police, which knows to eſtabliſh ſo ſpeedy and effectual a communication through mountains and deſarts for ſo long a way. The trade with Siberia is carried on by caravans to Seling, which is undoubtedly the Selinginſky of the Ruſſian travellers on the borders of Baykal lake. And this accounts for an extraordinary fact mentioned by BELL; that, on the banks of the river of that name, he one day found a man buſy in redeeming, from ſome boys who were angling, the fiſh they caught, and throwing them into the

the water again; and from this circumſtance, and the mark on his forehead, knew him to be an Indian. On converſing with him, he found his conjecture to be right. The man told him, he came from Madraſs, had been two years on his journey, and mentioned by name ſome of the principal Engliſh gentlemen there. This Indian, no doubt, muſt have travelled as a Faquier or Sunniaſſy through Bengal into Thibet, and from thence paſſed with the caravan to Selinginſky, where BELL found him. It is proper to remark, that the Indians have an admirable method of turning godlineſs into great gain, it being uſual for the Faquiers to carry with them, in their pilgrimages from the ſea-coaſts to the interior parts, pearls, corals, ſpices, and other precious articles, of ſmall bulk, which they exchange on their return for gold-duſt, muſk, and other things of a ſimilar nature, concealing them eaſily in their hair and in the cloths round their middle, and carrying on, conſidering their numbers, no inconſiderable traffic by theſe means. The Goſſeigns are alſo of a religious order, but in dignity above the Faquiers; and they drive a more extenſive and a more open trade with that country.

A particular account of the commerce would be foreign to the purport of this letter; but, as it would leave the information which I wiſh to convey very incom-

 pleat,

pleat, did I not mention the ſources from which this country, ſo apparently poor and unfruitful, draws a ſupply of the foreign articles of convenience and luxury, which I have occaſionally ſaid they poſſeſs; I ſhall juſt obſerve, that, beſides their leſs traffic with their neighbours in horſes, hogs, rock-ſalt, coarſe cloths, and other articles, they enjoy four ſtaple articles, which are ſufficient in themſelves to procure every foreign commodity of which they ſtand in need; all of which are natural productions, and deſerve to be particularly noticed. The firſt, though the leaſt conſiderable, is that of the cow-tails, ſo famous all over India, Perſia, and the other kingdoms of the Eaſt. It is produced by a ſpecies of cow or bullock different from what I believe is found in any other country. It is of a larger ſize than the common Thibet breed, has ſhort horns, and no hump on its back. Its ſkin is covered with whitiſh hair of a ſilky appearance; but its chief ſingularity is in its tail, which ſpreads out broad and long, with flowing hairs, like that of a beautiful mare, but much finer and far more gloſſy. Mr. BOGLE ſent down two of this breed to Mr. HASTINGS, but they died before they reached Calcutta. The tails ſell very high, and are uſed, mounted on ſilver handles, for Chrowras, or bruſhes, to chaſe away the flies; and no man of conſequence in India ever goes out, or ſits in

form

form at home, without two Chowrawbadars, or bruſhers, attending him, with ſuch inſtruments in their hands.

The next article is the wool from which the Shaul, the moſt delicate woollen manufacture in the world, ſo much prized in the Eaſt, and now ſo well known in England, is made. Till Mr. BOGLE's journey our notions on that ſubject were very crude and imperfect. As the Shauls all come from Caſſemire, we concluded the material from which they were fabricated to be alſo of that country's growth. It was ſaid to be the hair of a particular goat, the fine under hair from a camel's breaſt, and a thouſand other fancies; but we now know it for certain to be the produce of a Thibet ſheep. Mr. HASTINGS had one or two of theſe in his paddock when I left Bengal. They are of a ſmall breed, in figure nothing differing from our ſheep, except in their tails, which are very broad; but their fleeces, for the fineneſs, length, and beauty of the wool, exceed all others in the world. The Caſſemirians engroſs this article, and have factors eſtabliſhed for its purchaſe in every part of Thibet, from whence it is ſent to Caſſemire, where it is worked up, and becomes a ſource of great wealth to that country, as well as it is originally to Thibet.

Muſk is another of their ſtaple articles, of which it will be needleſs to ſay much, as the nature, quality, and value,

value of this precious commodity are ſo well known in Europe. I ſhall only remark, that the deer which produces it is common in the mountains; but being exceſſively ſhy, and frequenting ſolely the places the moſt wild and difficult of acceſs, it becomes a trade of great trouble and danger to hunt after. We have the muſk ſent down to Calcutta in the natural bag, not without great riſk of its being adulterated; but ſtill it is far ſuperior to any thing of the kind that is to be met with in ſale in Europe.

The laſt of the articles which I reckon ſtaple is gold, of which great quantities are exported from Thibet. It is found in the ſands of the Great River, as well as in moſt of the ſmall brooks and torrents that pour from the mountains. The quantity gathered in this manner, though conſiderable with reſpect to national gain, pays the individual but very moderately for the labour beſtowed on it. But, beſides this, there are mines of that metal in the northern parts, which are the reſerved property of the Lama, and rented out to thoſe who work them. It is not found in ore, but always in a pure metallic ſtate (as I believe it to be the caſe in all other mines of this metal) and only requires to be ſeparated from the ſpar, ſtone, or flint, to which it adheres. Mr. HASTINGS had a lump ſent to him at Calcutta, of about the ſize of

a bullock's

a bullock's kidney, which was a hard flint veined with ſolid gold. He cauſed it to be ſawed in two, and it was found throughout interlarded (if I may be allowed the expreſſion) with the pureſt metal. Although they have this gold in great plenty in Thibet, they do not employ it in coin, of which their government never ſtrikes any; but it is ſtill uſed as a medium of commerce, and goods are rated there by the purſe of gold-duſt, as here by money. The Chineſe draw it from them to a great amount every year, in return for the produce of their labour and arts.

I could wiſh to add to this account ſomething reſpecting the plants and other botanical productions of this country; but I would not preſume to offer any thing but what is authentic and exact, as far as my knowledge goes. Mr. BOGLE will no doubt be able to ſatisfy the learned in that branch, reſpecting many things of which I have at preſent no information. He ſent down to Calcutta many ſeeds, grains, kernels, and fruits, part of which only arrived ſafe. Of the laſt I taſted ſeveral, they were chiefly of the European ſorts, ſuch as peaches, apples, pears, &c. and therefore more deſirable for us in Bengal; but they were all to me inſipid and bad.

I am now, SIR, to cloſe theſe remarks with craving your forgiveneſs for having thus ſtarted a new ſubject of

curiofity, without the means of giving more compleat light concerning it. Time and opportunity may put more in my power on my return to India. In the mean time, I hope the Society will accept as a rarity the tranflation of the original letter which the Tayfhoo Lama wrote to Mr. HASTINGS, by the envoy whom he fent to folicit a peace for the Deb Rajah. It came into my hands in the courfe of my office, and by the permiffion of the Governor general I retained a copy.

The original is in Perfian, a language which the Lama was obliged to employ, that of Thibet, although very elegant and expreffive, as it is faid, being totally unintelligible in Bengal. A letter under the fanction of a character fo long talked of in the weftern world, but fo little known, alone renders it an object of curiofity; but, when it is found to contain fentiments of juftice, benevolence, and piety, couched in a fimple ftyle, not without dignity, and in general exempt from the high-flown compliments and ftrained metaphors fo common among the other people of the Eaft, I have no doubt of its being received with approbation; at any rate, it will ferve as a fpecimen of the way of thinking and writing among a people whofe country and manners I have made the fubject of the foregoing fketch.

Tranflation

Translation of a Letter from the Tayſhoo Lama *to Mr.* HASTINGS, *Governor of* Bengal, *received the* 29*th of* March, 1774.

THE affairs of this quarter in every reſpect flouriſh: I am night and day employed for the increaſe of your happineſs and proſperity. Having been informed, by travellers from your quarter, of your exalted fame and reputation, my heart, like the bloſſom of ſpring, abounds with ſatisfaction, gladneſs, and joy. Praiſe God that the ſtar of your fortune is in its aſcenſion. Praiſe him, that happineſs and eaſe are the ſurrounding attendants of myſelf and family. Neither to moleſt or perſecute is my aim; it is even the characteriſtic of our ſect to deprive ourſelves of the neceſſary refreſhment of ſleep, ſhould an injury be done to a ſingle individual; but in juſtice and humanity, I am informed you far ſurpaſs us. May you ever adorn the ſeat of juſtice and power, that mankind may, in the ſhadow of your boſom, enjoy the bleſſings of peace and affluence! By your favour I am the Rajah and Lama of this country, and rule over a

number of ſubjects; a particular with which you have no doubt been acquainted by travellers from theſe parts. I have been repeatedly informed, that you have been engaged in hoſtilities againſt the Dah Terria, to which it is ſaid the Dah's own criminal conduct, in committing ravages and other outrages on your frontiers, gave riſe. As he is of a rude and ignorant race, paſt times are not deſtitute of the like miſconduct which his avarice tempted him to commit. It is not unlikely but he has now renewed thoſe inſtances, and the ravages and plunder which he may have committed on the ſkirts of the Bengal and Bahar provinces, have given you provocation to ſend your vindictive army againſt him. However, his party has been defeated, many of his people have been killed, three forts have been taken from him, and he has met with the puniſhment he deſerved. It is as evident as the Sun that your army has been victorious; and that, if you had been deſirous of it, you might in the ſpace of two days have entirely extirpated him, for he had not power to reſiſt your efforts. But I now take upon me to be his mediator; and to repreſent to you, that, as the ſaid Dah Terria is dependant upon the Dalai Lama, who rules in this country with unlimited ſway (but, on account of his being in his minority, the charge of the govern-

ment

ment and adminiſtration for the preſent is committed to me) ſhould you perſiſt in offering further moleſtation to the Dah's country, it will irritate both the Lama and all his ſubjects againſt you. Therefore, from a regard to our religion and cuſtoms, I requeſt you will ceaſe all hoſtilities againſt him; and in doing this you will confer the greateſt favour and friendſhip upon me. I have reprimanded the Dah for his paſt conduct; and I have admoniſhed him to deſiſt from his evil practices in future, and to be ſubmiſſive to you in all things. I am perſuaded he will conform to the advice which I have given him; and it will be neceſſary that you treat him with compaſſion and clemency. As to my part, I am but a Faquier[(a)]; and it is the cuſtom of my ſect, with the roſary in our hands, to pray for the welfare of mankind, and for the peace and happineſs of the inhabitants of this country; and I do now, with my head uncovered, intreat that you may ceaſe all hoſtilities againſt the Dah in future. It would be needleſs to add to the length of this letter, as the bearer of it, who is a Goſeign[(b)], will repreſent to

(a) The original being in Perſian, this word is uſed, which can only be applied with propriety to a perſon of the Muſſulman faith: here it can only mean a religious perſon in general. Perhaps monk would have been the beſt tranſlation.

(b) This means a religious perſon of the Hindoo ſect.

you all particulars; and it is hoped you will comply there-with. In this country, worfhip of the Almighty is the profeffion of all. We poor creatures are in nothing equal to you; having, however, a few things in hand, I fend them to you by way of remembrance, and hope for your acceptance of them.

XXVI. *Of the Degrees and Quantities of Winds requisite to move the heavier Kinds of Wind Machines. In a Letter from* John Stedman, *M. D. Fellow of the Royal College of Physicians at Edinburgh, to the Reverend* Samuel Horsley, *LL.D. Secretary to the Royal Society.*

SIR,

Edinburgh, March 27, 1777.

Read Apr. 24, 1776.

THE irregularity and uncertainty of winds in this country have been found a considerable discouragement to erect wind machines. It hath frequently happened, that the proprietors of coal and other works, after having reared these kinds of engines, and having found them insufficient for the intended work, have been obliged to open mines, or to erect fire machines. This is chiefly owing to the undertakers reckoning upon more winds of a sufficient power to move these machines, than we commonly have in this country.

These machines are rarely erected with us, unless where a considerable moving power is necessary. This is always the case where the larger kind of pump-work is to be kept in motion, or where water is to be extracted

from

from deep pits. Having enquired of many people concerned in ſuch works, what may be the proportion of time in which wind machines may be kept in motion, to that in which they cannot move from a defect of wind, I found theſe people differing widely in their conjectures.

Having, however, met with one gentleman of obſervation and accuracy, who had erected a wind machine for draining his coal; he told me that, by the beſt computation he had been able to make, he never could depend upon more than fifty-three or fifty-four hours of wind ſufficient for moving that machine, in a week, taking the year round. This is below what is commonly believed to be the proportion; but, ſo far as this can be rated by an eſtimate in the following manner, it will be found to be much about what may be depended upon for the heavieſt kinds of machines; ſtill making allowance for the differences of expoſures, and for the ſtrength and frequency of winds in one part of the country more than in another. We may here take notice of a circumſtance favourable to the draining of coal-pits; that is, that the periods of the year in which the greateſt quantities of rain fall, are likewiſe obſerved to abound with winds of the higher degrees. This ſeldom fails to hold in hilly countries, and particularly in thoſe of high latitudes; that is, where the differences of heats in ſummer

 and

and of colds in winter are very confiderable. The periods of the year here meant will generally be found to fall near the Equinoxes.

The only method of bringing the matter to a proper eftimate is, by comparing the quantities of winds fufficient for moving thefe machines, with thofe of winds below that degree, and calms. This computation can only be drawn from journals in which the degrees of winds are noted. In the meteorological regifter of the Medical Effays of the Edinburgh Society, there is a column of winds, and four degrees are noted. This divifion is fufficient for the purpofes for which that regifter was intended; but, when we confider the wind as a power acting upon machines, that number of degrees will be found too fmall.

Thus, from the fecond in that regifter to a hurricane, there is but one intermediate degree. As the fecond degree, which is very moderate, is infufficient for moving thefe machines; the third is more than juft enough for that purpofe. A degree, therefore, which is a mean betwixt thefe two, will be found to be the loweft that is fufficient for moving machines of the heavier kinds, particularly fuch as are ufed for pumping water out of coal-pits.

Thefe

These three degrees of wind, that is the second and third of the Edinburgh regifter, and an intermediate degree, are very diftinguifhable even by the fenfes, and without the affiftance of any inftrument, by thofe who are attentive and have been accuftomed to make obfervations of this nature.

To afcertain proportions of this nature, a longer term of years would have been more fatisfactory; but, in cafe others fhould afterwards purfue this kind of computation, the proportions are digefted in two tables at the end of this effay, and may be confulted occafionally.

In making up thefe tables, *viz.* one of the fecond degree and above, and the other of the third and upwards, hurricanes are included, though that degree of wind be too high for any machine. But, as the obfervations were taken twice in twenty-four hours, and as winds fufficient to move thefe machines may be fuppofed to have happened fometimes between the times of obfervation, though at thefe times the wind might have been below the mean; to compenfate this defect, hurricanes are included in the computation.

From thefe tables then we have the following proportions of the two degrees of winds and upwards, to thofe below; and likewife of the mean betwixt thofe two degrees.

Winds

	Days.
Winds of the second degree and upwards in each week, — —	4.283
Calms and winds below the second degree,	2.717
Winds of the third degree and upwards,	0.902
Calms and winds below the third degree,	6.098
Winds of a mean proportion between the two preceding degrees, — —	2.592
Calms and winds below the mean degree,	4.408

The proportion of these winds in the year comes out in weeks and fractional parts thus:

	Weeks.
Winds of the second degree and upwards in the year, — — —	31.903
Calms and winds below the second degree,	20.239
Winds of the third degree and upwards,	6.719
Calms and winds below the third degree,	45.423
Winds of a mean proportion between the two preceding, — — —	19.307
Calms and winds below the mean, —	32.835

From this computation we have 2.592 days in a week, or 19,307 weeks in a year, in which wind machines of the

the heavier kinds, and of confiderable friction, may be fuppofed to be kept in motion; which, to the times wherein they cannot go, is as 10 to 17.

It may be obferved, that the refiftance to the machine, or its weight and friction, being diminifhed, though in a fmall degree, will add confiderably to the frequency and length of times in which it can go; fince it often happens that there are winds immediately below the loweft degree in the preceding eftimate, fufficient to keep the lighter machines in motion. Hence thofe who have machines which are not abfolutely of the heavieft kind, will be apt to conclude this computation erroneous. Befides, there are few who make allowance for, or attend to, the univerfal law which obtains in mechanics, that in larger machines, their power doth not increafe in a proportion fo high as their bulk and the refiftance arifing from their friction.

Computations of this nature, if carried on for a fufficient length of time, might be of fome ufe in regulating infurances, or in pointing out the rifks of nautical adventurers, when made in the fame climates with the calculation of winds.

Here I fhould have concluded; but having, after writing what is above, committed thefe tables and obfervations to the examination of a learned member of your So-

 ciety,

ciety, much conversant in these matters, I had the satisfaction to find that he thought them worthy to be communicated to the Royal Society; but remarked, that the materials, which I had proceeded upon, were not so applicable to the purpose as could have been wished. He thinks, that the degrees of winds, being only distinguished into four in the journals from whence those tables have been compiled, are much too few to take in those of the weaker kind, that will however turn well-constructed wind-mills. Indeed I regretted that the table from which I made my estimate contained so few degrees; but it was for that reason I calculated an intermediate degree between the second and the third of our meteorological register. Now as all the degrees above that intermediate degree are sufficient to move the heaviest machine, and the degrees below it insufficient for that purpose, so far as I have been able to observe, it comes to be the same as if the table, from which I made the estimate, had consisted of eight degrees, supposing a mean proportion to be found between the other degrees: thus, 0, $\frac{1}{2}$, 1, $1\frac{1}{2}$, 2, $2\frac{1}{2}$, &c. this last number $2\frac{1}{2}$, being the lowest degree which I find sufficient for the heaviest machines, would have been 5, had these fractional parts been integers in the table, so that the highest degree, instead of 4 would have been 8. The mean, therefore,

 between

between 2 and 3 being found, will, if I miſtake not, anſwer the preceding objection.

This worthy member, at the end of his obſervations, ſays, a mill ſo conſtructed may be expected to go the half of the year; that is, I preſume, a wind-mill conſtructed in the neateſt and moſt ingenious manner. But this, I have reaſon to believe, is far from being the caſe with wind-mills in this country, they being for the moſt part clumſy. I doubt not but wind-mills, the conſtruction of which this ingenious gentleman hath directed, though of the ſame ſize and conſiſting of the ſame numbers with thoſe I have ſeen here, will nevertheleſs be moved by a lower degree of wind, and conſequently will go a greater proportion of time, though they have the ſame reſiſtance to overcome as others leſs artificially conſtructed. Indeed the ſame wind machine, as is well known, will require a degree of wind conſiderably higher when its joints are dry or become gummy, than when they are ſufficiently greaſed. In my eſtimate I have all along had an eye to the wind machines which have the greateſt reſiſtance to overcome, and conſequently the machines themſelves of the largeſt kind. But when the learned gentleman ſuppoſes a machine to go one half of the year, he may perhaps not underſtand one abſolutely of

the

the largest kind. Now as the powers of similar machines, but of different magnitudes, are as their cubes, but the resistance arising from their weight and friction as the fourth power; it follows, that a small difference in the size of two or more wind machines will require considerable differences in the winds necessary for their movements.

I am, &c.

TABLE I.

Shewing the Proportion of winds of the fecond degree and upwards, to thofe of the firft and below, for five years.

	Proportion of winds of the 2d degree to thofe of the firft, &c. in 1731 and 1732.		Proportion, &c. in 1732 and 1733.		Proportion, &c. in 1733 and 1734.		Proportion, &c. in 1734 and 1735.		Proportion, &c. in 1735 and 1736.		Sum of each month for five years.	
	Winds.	Calms, &c.	Winds.	Calms, &c.	Winds.	Calms, &c.	Winds.	Calms, &c.	Winds.	Calms, &c.	Winds.	Calms, &c.
June	16	44	46	14	26	34	24	36	41	19	153	147
July	7	55	44	18	39	23	29	33	43	19	162	148
Auguft	8	54	27	35	20	42	28	34	47	15	130	180
September	30	30	24	36	40	20	34	26	34	26	162	138
October	26	36	13	49	36	26	31	31	35	27	141	169
November	39	21	18	42	51	9	16	44	33	25	157	141
December	43	19	29	33	38	24	46	16	41	21	197	113
January	55	7	43	19	35	27	58	4	45	17	236	74
February	45	13	46	10	48	8	47	9	42	16	228	56
March	39	23	50	12	40	22	53	9	46	16	228	82
April	44	16	34	26	42	18	46	14	46	14	212	88
May	54	8	28	34	36	26	55	7	57	5	230	80
Sum of years and months,	406	326	402	328	451	279	467	263	510	220	2236	1416

TABLE

TABLE II.

Shewing the proportion of winds of the third degree and upwards, to thoſe of the ſecond and below, for five years.

	Proportion, of winds of the 3d degree to thoſe of the firſt, &c. in 1731 and 1732.		Proportion, &c. i. 1732 and 1733.		Proportion, &c. in 1733 and 1734.		Proportion, &c. in 1734 and 1735.		Proportion, &c. in 1735 and 1736.		Sum of each month for five years.	
	Winds.	Calms, &c.	Winds.	Calms, &c.	Winds.	Calms, &c.	Winds.	Calms, &c.	Winds.	Calms, &c.	Winds.	Calms, &c.
June	1	59	–	60	3	57	2	58	1	59	7	233
July	1	61	7	55	11	51	5	57	1	61	25	285
Auguſt	–	62	–	62	8	54	3	59	–	62	48	252
September	13	47	9	51	10	50	8	52	8	52	11	299
October	9	53	1	61	12	50	11	51	2	60	35	275
November	11	49	8	52	19	41	2	58	6	54	46	254
December	17	45	4	58	12	50	9	53	4	58	46	264
January	5	57	11	51	9	53	18	44	6	56	49	261
February	24	34	20	36	24	32	12	44	7	51	87	197
March	12	50	10	52	18	44	6	56	5	57	51	259
April	9	51	5	55	9	51	8	52	5	55	36	264
May	7	55	–	62	9	53	10	52	4	58	30	280
Sum of years and months,	109	623	75	655	144	586	94	636	49	683	471	3183

XXVII. *De-*

XXVII. *Description of the Jesuits Bark Tree of* Jamaica *and the* Caribbees. *By* William Wright, *M.D. Member of the Philosophical Society of* America, *and Surgeon-general in* Jamaica. *Communicated by* Joseph Banks, *Esquire, F. R. S.*

Read April 24, 1777.

THIS species of Jesuits bark grows on stony lands near the sea-shore, in the parishes of St. James and Hanover, on the north-side of Jamaica; and I found one small tree, at a little distance from the fort, at Martha Brae in the parish of Trelawny. The tree is called the Sea-side Beech, and rises only to twenty feet. The trunk is not thick in proportion, but hard, tough, and of a yellowish-white colour in the inside. The branches and leaves are opposite; the leaves are of a rusty green, and the young buds of a blueish green hue. It blossoms in November, and continues in flower till February, having on the same tree or sprig, flowers and ripe pods. The flowers are of a duskish yellow colour, and the pods black: when ripe they split in two, and are, with their flat brown seeds, in every respect similar to those

thoſe of the *Cinchona officinalis* as depicted in a plate ſent out by Mr. BANKS.

The bark of this tree in general is ſmooth and grey on the outſide, though in ſome rough and ſcabrous. When well dried, the inſide is of a dark-brown colour. Its flavour at firſt is ſweet, with a mixture of the taſte of horſe-radiſh and of aromatics of the Eaſt; but, when ſwallowed, of that very bitterneſs and aſtringency which characteriſes the Peruvian bark. It yields theſe qualities ſtrongly to water both when cold and in decoction. Half an ounce, boiled from two pounds to one pound of water, made as ſtrong a decoction as three times its weight of the *Cinchona vera*. The colour was brown, but not turbid.

I have had many opportunities of trying its effects, eſpecially in remittents, which are the moſt common and fatal fevers in theſe climes. A vomit or gentle purge, if neceſſary, was firſt given; and then immediately this bark ſo ſoon as they operated. I obſerved that it ſtrengthened the ſtomach, checked retching and vomiting, corrected morbid humours in *primæ viæ*, and conquered ſpeedily the diſeaſe. My ſucceſs in ſuch a dangerous malady leaves not a doubt on my mind, but that it will prove equally efficacious in every other caſe where a tonic and antiſeptic medicine is indicated.

CINCHONA JAMAICENCIS, ſeu CARIBBEANA.

CINCHONA CARIBÆA, *Linn. Spec. Plant.* 245.

FOL. ovata, integerrima, acuta, enervia, oppoſita.

FLOR. ſingulares, axillares.

CAL. *Perianthium* monophyllum, ſuperum, quinquefidum, minimum, perſiſtens, campanulatum, obſoletiſſimè quinquedentatum.

COR. monopetala, infundibiliformis. *Tubus* cylindraceus, longiſſimus: *Limbus* quinquepartitus, tubo equalis: *Laciniis* ovatis, oblongis, reflexis, quandoque pendulis.

STAM. *Filamenta* quinque, filiformia, erecta e medio tubi, longitudine corollæ. *Antheræ* longiſſimæ, obtuſæ, erectæ ſupra baſin exteriorem, affixæ in fauce corollæ.

CAPS. bipartibilis in duas partes diſſepimento paralello, latere inferiore dehiſcens.

SEM. plurima, compreſſa, marginata, oblonga.

Cinchona Jamaicensis

Basire sc.

XXVIII. *Description and Use of the Cabbage-bark Tree of* Jamaica. *By* William Wright, *M. D.*; *communicated by* Richard Brocklefby, *M. D. F. R. S.*

Read May 1, 1777.

THE Cabbage-bark tree, or Worm-bark tree, grows in moft parts of Jamaica, and particularly abounds in the low Savannahs of St. Mary and St. George. It rifes to a confiderable height, but no great thicknefs, fending off branches towards the top of a ftraight, fmooth trunk. The leaves are, when young, of a light-green hue; when full-grown, of a dark-green colour; and before they drop, of a rufty appearance.

The flower-fpike is long and beautifully branched. The flowers are numerous; their *calyces* of a dark purple; their *petals* of the colour of the pale-rofe; the *nectaria* muft contain much honey, as thoufands of bees, beetles of various kinds, butter-flies, and humming-birds, are continually feeding thereon.

The *pericarpium* is a green, hard fruit, of the fize of the fmaller plumb. The fkin is of the thicknefs of a crown-piece; and taftes very auftere. The kernel is covered with a brown fkin like that of other nuts; it is very hard, and taftes aftringent.

XXVIII. *Description and Use of the Cabbage-bark Tree of* Jamaica. *By* William Wright, *M. D.*; *communicated by* Richard Brocklesby, *M. D. F. R. S.*

Read May 1, 1777.

THE Cabbage-bark tree, or Worm-bark tree, grows in most parts of Jamaica, and particularly abounds in the low Savannahs of St. Mary and St. George. It rises to a considerable height, but no great thickness, sending off branches towards the top of a straight, smooth trunk. The leaves are, when young, of a light-green hue; when full-grown, of a dark-green colour; and before they drop, of a rusty appearance.

The flower-spike is long and beautifully branched. The flowers are numerous; their *calyces* of a dark purple; their *petals* of the colour of the pale-rose; the *nectaria* must contain much honey, as thousands of bees, beetles of various kinds, butter-flies, and humming-birds, are continually feeding thereon.

The *pericarpium* is a green, hard fruit, of the size of the smaller plumb. The skin is of the thickness of a crown-piece; and tastes very austere. The kernel is covered with a brown skin like that of other nuts; it is very hard, and tastes astringent.

The wood is hard, and takes a good polifh. It is however fit only for rafters or other parts of fmall buildings; but this tree is valued chiefly for its bark, which externally is of a grey colour, and the infide black and furrowed.

Frefh cabbage-bark taftes mucilaginous, fweet, and infipid. Its fmell, however, is rather difagreeable, and it retains it in the decoction; hence by fome called the bulge-water tree.

Mr. PETER DUGUID, formerly of this ifland, feems to have been the firft that gave any account of the virtues of this bark, in the Edinburgh Effays, Phyfical and Literary, vol. II. The experiments he promifed have never yet appeared. It is certain it has powerful effects, and its anthelmintic quality is eftablifhed by the experience of feveral ages. It is at prefent in general ufe here, and begins to be known in Europe. No defcription having yet appeared, I have fupplied that defect as far as my abilities in Botany reached. It remains now to proceed to its exhibition, and the purpofes it is meant to anfwer as a medicine.

Cabbage-bark may be given in different forms; as in decoction, fyrup, powder, and extract. I have ufed them all, and fhall fpeak of them feparately.

The decoction. Take fresh-dried or well-preserved cabbage-bark, one ounce. Boil it in a quart of water, over a slow fire, till the water is of an amber colour, or rather of deep coloured Madeira wine; strain it off, sweeten it with sugar, and let it be used immediately, as it does not keep many days.

Syrup of Cabbage-bark. To any quantity of the above decoction add a double portion of sugar, and make a syrup. This will retain its virtues for years.

The extract of cabbage-bark is made by evaporating the strong decoction in *balneo mariæ* to the proper consistence; it must be continually stirred, as otherwise the resinous part rises to the top, and on this probably its efficacy depends.

The powder of well-dried bark is easily made, and looks like jallap, though not of equal specific gravity.

This bark, like most other powerful anthelmintics, has a narcotic effect; and on this account it is always proper to begin with small doses, which may be gradually increased till a nausea is excited, when the dose for that patient is ascertained. But by frequent use we can in common determine the dose, though we chuse to err rather on the safe side.

A strong healthy grown person may, at first, take four table spoonfuls of the decoction or syrup, three grains of the extract, or thirty grains of the powder for a dose.

A youth

A youth, three table fpoonfuls of the decoction or fyrup, two grains of extract, or twenty grains of powder.

A perfon of ten years of age, two table fpoonfuls of the decoction or fyrup, one grain and a half of extract, or fifteen grains of the powder.

Children of two or three years old, a table fpoonful of the decoction or fyrup, one grain of extract, or ten grains of the powder. Children of a year old, half the quantity.

Thefe may be increafed, as above obferved, till a naufea is excited, which will depend on the ftrength, fex, and habit of body of the patient.

Care muft be taken that cold water be not drank during the operation of this medicine, as it is in this cafe apt to occafion ficknefs, vomiting, fever, and delirium. When this happens, or when an over large dofe has been given, the ftomach muft be wafhed with warm water: the patient muft fpeedily be purged with Caftor-oil and ufe plenty of lime-juice beverage for common drink; vegetable acid being a powerful antidote in this cafe, as well as in an over dofe of opium.

The decoction is what is moftly given here, and feldom fails to perform every thing that can be expected from an anthelmintic medicine, by deftroying worms in the inteftines, and bringing them away in great quantities. By frequent ufe, however, thefe animals become fami-

liarized, and we find it neceſſary to intermit it, or have recourſe to others of inferior merit.

The writers of the Edinburgh Medical Commentaries take notice, that the decoction of cabbage-bark always excites vomiting. We find no ſuch effect from it here, and may account for it by their receiving it in a mouldy ſtate. A ſyrup, therefore, is given there with better effect. They obſerve alſo that it has a diuretic virtue, which we have not taken notice of here.

This bark purges pretty briſkly, eſpecially in powder, thirty or forty grains working as well as jallap by ſtool; but in this way it does not ſeem to kill worms ſo well as in decoction.

Five grains of the extract made a ſtrong man ſick, and purged him ſeveral times; but, by frequent uſe, he took ten grains to produce at length the ſame effect.

It muſt not be concealed that fatal accidents have happened from the imprudent adminiſtration of this bark, chiefly from over-doſing the medicine. But this cannot detract from the merit of the cabbage-bark, ſince the beſt medicines, when abuſed, become deleterious; and even our beſt aliments, in too great quantity, prove deſtructive. Upon the whole, the cabbage-bark is a moſt valuable remedy, and I hope will become an addition to the *materia medica.*

GEOFFRÆA JAMAICENCIS INERMIS.

FOL. opposita, oblongo-ovata, ternata, acuminata, superne glabra, inferne enervia, petiolis brevibus.

CAL. *Perianthium* monophyllum, campanulatum, leviſſimè quinquepartitum, *laciniis* ovatis, brevibus.

COR. papilionacea: *Vexillum* ſubrotundum, concavum: *Alæ* obtuſæ, concavæ, longitudine vexilli. *Carina* ovata, patens, in duabus partibus leviſſimè diviſa.

STAM. diadelpha, decem, filiformia, in calyce inſerta, longitudine alarum. *Antheræ* ſubrotundæ.

PIST. ſubulatum, filiforme. *Stigma* nullum. *Germen* ovato-oblongum, compreſſum.

PER. *Drupa* ſub-ovata, magna.

SEM. *Nux* ſub-ovata, ſub-lignea, ſulco utrinque longitudinali, bivalvis.

The botanical reader will ſee how nearly this agrees with the *Geoffræa ſpinoſa* of LINNÆUS. The Genera of plants are ſufficiently multiplied, and it was thought beſt to make this a ſpecies only.

Geoffræa jamaicensis inermis

J. Basire Sculp

made expreſsly in London and Paris, in order to make ſuch experiments as might preſent themſelves to me *en courrant*; and which, either from want of acquaintance with the ſubject, want of time, or want of money, become rarely the object of travellers; but remain wholly unknown till princely munificence and philoſophic zeal (of which we have a recent inſtance) unite in producing them to the world. After the very celebrated and ingenious labours of Mr. DE LUC, farther inveſtigation of the ſubject of barometrical meaſurement might ſeem unneceſſary, if not invidious; but, furniſhed as I was with an apparatus every way ſufficient for the inquiry, finding myſelf in the country which had been the ſcene of his operations, and poſſeſſing ſome ſhare of his own zeal, I could not but gratify the curioſity I had to verify and repeat his experiments: if therefore in the purſuit of this inquiry I ſhould be led to a concluſion ſomething different from the reſult of his own obſervations, I am convinced that this diſtinguiſhed obſerver, of whoſe candour and talents I have an equal opinion, will impute it wholly to a love for truth; as with me the precept applies as ſtrongly to the philoſopher as to the hiſtorian, *Ne quid falſi audeat, ne quid veri non audeat dicere.*

But to proceed. The inftruments I made ufe of in thefe operations were, two of RAMSDEN's barometers (a); three or four thermometers detached from the barometers, whofe boiling and freezing points I had examined myfelf; an equatorial inftrument, the circles of which were about feven inches diameter, made by RAMSDEN; a fifty-feet fteel meafuring chain; and three three-feet rods, two of deal and one of brafs, in order to examine and correct the chain, thefe latter made by BARADELLE at Paris. Befides thefe I took with me a little bell-tent, which I found of great ufe, as it defended me from the wind and fun; and I may remark, that the obfervations of the uppermoft barometer were made in the tent.

My firft feries of obfervations I propofed to be on Mont Saleve (b), one of the Alps, fituated about two

(*a*) It may not be improper to remark, that the fpecific gravity of the quickfilver of thefe barometers with 68° of heat was 13,61; the diameter of the bore of the tube 0,20 inch; and that of the refervoir 1,5 inch.

(*b*) Mont Saleve extends near nine miles in length; is not quite 3300 feet in height above the Lake. That fide of it which is next Geneva is for the moft part a barren rock, the north-eaft end of it being almoft a perpendicular precipice; the other fide of the mountain is lefs rude, of a more gentle acclivity, covered with trees, fhrubs, and herbage, as is alfo the top, where is fome of the fineft pafture in the world. It is inhabited only by a few fhepherds, who pafs the fummer months here with their cattle, in little miferable huts or barns: the remaining part of the year, *viz.* for about four or five months, it is covered with fnow. This mountain contains chiefly a calcareous ftone; and there is reafon to believe that there is an iron ore in it, at leaft in fome parts of it, as a piece Mr. DE LUC, the brother, picked up near the fouth-weft end, I found, fenfibly affected the magnet.

leagues fouth of Geneva, and precifely on the fame point where Mr. DE LUC had made his higheft or fifteenth ftation: this fpot I learnt from his brother, whofe civilities, both then and fince, I fhall frequently have occafion to remember and mention.

The place where I meafured my bafe was in a field near the villages of Archamp and Neidens, not quite three miles in a horizontal line from the top of the rock whofe height was to be determined (fee the chart that accompanies this account). At the end of the bafe A I intended to place one of my barometers; and the other at the top of the rock, called the Pitton, at C; and with the above inftruments meafure the triangle ABC. The angles were taken both on the horary circle, which was brought parallel to the horizon, and alfo on the azimuth circle of the equatorial inftrument; this made it, as it were, two different inftruments independant of each other. The angles were moreover doubled, tripled, and quadrupled, on each arch; by this means the error of the center or axis of the inftrument vanifhed; the poffible error in the divifions, in the reading off, and in the coincidence of the wires in the telefcope (which magnified forty times) with the fignals placed at each angle of the triangle, was leffened in proportion to the number of times the obfervation was repeated; and finally the

mean

mean of all was taken. The ſame was done with each angle at A, B, and C, horizontal as well as vertical, *viz.* the elevation of C above A and B was taken; and alſo the depreſſion of A and B below C. The advantage of this method was, that the error of the line of collimation, the effect of refraction, and of the curvature of the earth's ſurface, all became equal and contrary; by theſe means the little errors were diminiſhed, and great errors abſolutely avoided[c]. I ſhall, however, beg leave to ſet down the operation at length reſpecting this one triangle, in order to ſhew the preciſion that may be expected from ſuch a geometrical proceſs; to remove the ſcruples of thoſe gentlemen who ſuſpect that accuracy is only to be obtained by large quadrants; and laſtly, to do juſtice and ſatisfaction to the celebrated artiſt who invented and made this valuable inſtrument.

(*c*) I muſt acknowledge here, that the attraction of the mountain creeps into the account uncorrected for, but only half of this quantity influences the mean reſult, as at the top it was nothing, and at the bottom of the mountain it could not exceed 10″ in the direction AC, as I find from a rough computation, the half of which = 5″ would give only four inches for the correction.

Determination of the Baſe.

	Ch.	Ft.	In.	Temper.
Length of the baſe AB (ſee the Chart) by the chain, firſt time, — — —	55	10	0	71°
Ditto, ſecond time, — — —	55	9	9¾	76
The mean, — —	55	9	10.87	73½
	Ft.	In.		
By frequent previous obſervations I determined (*d*) the length of the chain by compariſon with the braſs ſtandard rod reduced to 60° of heat, — —	50	0	0	60
Correction for 13½° of heat from expanſion, —	+0	0	05	
Diameter of the pins or arrows, one of which was uſed at each chain, and in ſuch manner, that this correction became always + — — —	+0	0	16	
Correct length of the chain as it was uſed in meaſuring the baſe, — — — —	50	0	21	
Multiply by the number of entire chains in the baſe, —			55	
	2750	11	55	
Add the parts of a chain, — —	+ 9	10	87	
True length of the baſe, as it was meaſured, —	2760	10	42	
Correction for the defect of level, taken with an inſtrument made on purpoſe, each time the chain was placed,	—	0	76 (*e*)	
The true horizontal diſtance between A and B becomes,	2760	9	66	

Deter-

(*d*) It may be required, to what preciſion I could determine the length of my chain? I think certainly to within $\frac{3}{100}$ of an inch, or $\frac{1}{20000}$ of the whole length. The common GUNTER's chain of the ſhops is always ſubject to ſpring and ſtretch conſiderably; mine was made of hardened ſteel, on purpoſe to avoid this defect. It however ſtill preſerved ſome degree of elaſticity, for when pulled with a force of about ten pounds, it ſeemed $=0,12$ inch longer than when laid gently on the floor without being ſtretched at all: the aſſumed length of the chain was ſuch as ſeemed to me probable from a moderate tenſion in common

Determination of the angles by the equatorial.

	On the azimuth circle.	On the equat. circle, the horary being converted into gradual divisions.
	° ′ ″	° ′ ″
∠ A by the 1ſt obſervation —	58 27 30	— 58 28 30
2d, — —	— 29 0	— — 27 30
3d, — —	— 28 30	— — 29 15
4th, — —	— 30 15	— — 29 15
∠ taken four times over on the arch, —	233 54 15	— 233 54 30
The mean, —	58 28 49	— 58 28 $37\frac{1}{2}$

Laſtly, the mean of all from the two circles $= 58° 28' 43\frac{1}{4}'' =$ ∠ at A.

∠ B by the 1ſt obſervation, —	111 54 45	— 111 53 0
2d, — —	— 51 30	— — 52 30
3d, — —	— 50 30	— — 50 45
∠ taken three times over on the arch, —	335 36 45	— 335 36 15
Mean, — —	111 52 15	— 111 52 5

Mean of all from the two circles $= 111° 52' 10''$ = ∠ at B.

common uſing it. It may perhaps not be out of place to remark here, that the rods with which the chain was examined, agreed exactly with the ſcales of the barometers; at leaſt the difference in nine inches, taken in different parts of the ſcale, did not appear to exceed $\frac{1}{1000}$ of an inch.

(e) The precaution in taking the inclination of the chain every time, if the baſe be nearly a plain, as is the caſe in many meadows, ſeems to be unneceſſary; for this ſame correction, deduced from the inclination of the baſe obſerved at A and B, comes out —0,99 inch, only 0,23 inch different, a quantity wholly inconſiderable.

∠ C

	On the azimuth circle.		On the equat. circle.
	° ′ ″		° ′ ″
∠ c by the 1st observation, —	9 39 0	—	9 38 30
2d, — —	– 39 0	—	– 38 15
3d, — —	– 38 45	—	– 39 45
∠ taken four times over on the arch, —	38 35 45	—	38 34 45
Mean, — —	9 38 $56\frac{1}{4}$	—	9 38 $41\frac{1}{4}$

Mean of the two circles, = $9^\circ\ 38'\ 48\frac{3}{4}''$ = ∠ at c.

	By actual observation.		Angles finally corrected.
	° ′ ″		° ′ ″
∠ at A, — —	58 28 $43\frac{1}{4}$	These angles corrected by adding 6″ to each (the sum of their errors, or defect from 180° being —18″) become,	58 28 $49\frac{1}{4}$
∠ at B, — —	111 52 10		111 52 16
∠ at C, — —	9 38 $48\frac{3}{4}$		9 38 $54\frac{3}{4}$
Sum of the three angles =	179 59 42	Sum,	180 0 0
Taken from	180 0 0		
Leaves the difference = sum of the errors,	— 18		

It is highly curious and satisfactory to see the amazing correspondency of these observations, made with an instrument of only $3\frac{1}{2}$ inches radius, whereon an angle of one minute is about equal $\frac{1}{1300}$ inch; and I think we may fairly conclude, that the corrected mean result of these observations is true to within 6″ or 8″(f); which, as

(f) I may have a future occasion to speak of the accuracy of this instrument for astronomical purposes; but I cannot omit this opportunity of mentioning one, *viz.* in taking the latitude of the city of Amiens in Picardy, where I had thirteen observations by the stars and Sun, the mean of which differed 25″ from the extremes, and only 3″ from the result of Mr. CASSINI's observations, made, I believe, with a nine-feet zenith sector, as related in *La Meridienne de Paris verifiée.*

5

may

may be proved hereafter, would occaſion an error of only three feet in the diſtance of the mountains, and ſeven inches in the height. I proceed next to the vertical angles.

Determination of the inclination of the ſides AC, BC, *and* AB, *with the horizon; the height of the eye at the inſtrument being four feet above the ground.*

Altitude from below at A.	°	′	″
Inclination of AC, —	10	33	2
Correction for the part of the ſignal which was obſerved,		— 1	38
Correction for the line of collimation, —		— 0	59
Correct for the refraction,		— 0	27
True Altitude of C from A,	10	29	58

Depreſſion from above at C.	°	′	″
	10	29	18
Correct for the ſignal, —			+ 16
——— for the line of collimation, —			+ 59
——— for refraction,			+ 27
True depreſſion of A from C,	10	31	0
Arch intercepted between, or curvature, —		— 2	30
True altitude of C from A deduced from the obſervation at C,	10	28	30

Mean corrected altitude of C from A = $10^\circ\ 29'\ 14''$ [g].

(g) If the computation were to be made from either of the obſervations taken ſeparately, the difference would amount to only three feet in the height of C; and this may either be in the correction of the line of collimation, the effect of refraction, or in miſtaking the part of the ſignal that was obſerved; for, whilſt I was gone to the top of the mountain, ſome peaſants poſſeſſed themſelves of the handkerchiefs I had fixed to the ſignals below in order to have a conſpicuous and determined point.

Altitude from below at B.	°	′	″
Inclination of BC, —	11	20	26
Correct for the part of the signal observed, —	—	1	38
Error of collimation,	—	0	59
Correct for refraction,	—	0	26
True altitude of C from B,	11	17	23

Depression from above at C.	°	′	″
	11	19	47
Correct for the signal, —		—	59
Error of collimation, —		+	59
Effect of refraction, —		+	26
True depression of B from C,	11	20	18
Arch intercepted, or curvature, —		— 2	18
True altitude of C from B, deduced from the observation at C, —	11	18	0

Mean of the two, or corrected altitude of C from B $= 11^\circ\ 17'\ 41\frac{1}{2}''$.

Altitude at A.	°	′	″
∠ of inclination of AB the base, —	0	27	0
Error of the line of collimation, —	—	0	59
Correct altitude of B from A,	0	26	1

Depression at B.	°	′	″
	0	27	4
Error of collimation,	+	0	59
Correct depression of A from B, —	0	28	3
Arch intercepted, —	—	0	27
Altitude of B from A deduced from the observation at B, —	0	27	36

Mean of the two, or corrected altitude of B from A $= 0^\circ\ 26'\ 49''$(b).

(b) It should seem from these two observations, that the error of the line of collimation had been assumed too great; it has however, as I have before observed, nothing to do with the mean result: and this is, perhaps, one of the best means of discovering the error of collimation, and the very method Mr. DE LUC used, to adjust his level, though, as I have been informed by his brother, without taking into the account the effect of curvature, which, if his horizontal marks were 2000 feet distant from each other, would amount to 20″, and the error to half that quantity.

 I have

I have thus, in a manner rather prolix, given a detail of the methods used to ascertain the quantity of the different angles. It may be of use on a like occasion, and will at least serve to determine within what limits the error of the final result may be expected to lye, as on the precision of the geometrical operations all the comparisons of the barometrical ones depend. This process once mentioned will exempt me and the reader from the trouble a second time, when he is informed, that the same fidelity and pains were employed (where the circumstances would admit) in all the trigonometrical observations, of which the annexed chart is a summary. I proceed now to the determination of the sides, the computations of which are too well known to enter into this paper.

	Feet.
Side AB	2760.8
AC	15286.4
BC	14041.7

	Feet.
These with the angles give for the height of C above A, —	2835.07
The height of C above B, — — —	2806.27
The height of B above A, — — —	22.18
These two added give the height of C above A, deduced from the observation at B, — — —	2828.45
But the height by actual observation at A was, — —	2835.07
Then the mean of the two, — — —	2831.76

which is probably within three or four feet of the truth, or about one foot in a thousand.

Having

Having thus the perpendicular height, as I think, very accurately aſcertained, it remained for me to take the altitude of the barometer at each ſtation A and C, and if poſſible with equal preciſion. Theſe obſervations it would be too tedious to ſet down at length. I ſhall, however, premiſe the following particulars. Every obſervation of the barometer was triple; that is, the height was read off three different times, and the mean taken; but from once reading only I could be ſure of the height to $\frac{2}{1000}$ of an inch, excluſive of the error of the diviſions, which in ſome places might amount to that quantity; this the nonius would itſelf diſcover and even correct by eſtimation. At every ſeries of obſervations the float at the bottom was readjuſted, ſo that I could conſtantly be ſure of an alteration of the weight of the atmoſphere expreſſed by 0.002 inch of quickſilver, if not of half that quantity. Finally, the difference of the two barometers (i) was conſtantly taken, after being left three-quarters

(i) It may be concluded, that this difference ſhould be conſtant, and always the ſame; but, from what cauſe I know not, it did not appear ſo to me. In my journal for the weather for 1775, I find the following note: from a mean of ſeventeen obſervations between Auguſt 12th and Sept. 1. *viz.* before, at, and after, my expedition to Mont Saleve and the Mole, I find the difference between my two barometers =,0042 inch, N° 1. ſtanding the higheſt; in theſe compariſons, however, the extremes ſometimes differed from the mean =,006. And in my paſſage over Mont Cenis, Dec. 1. barometer N° 1. ſtood lower than N° 2. by

ters of an hour or more in the same place, to acquire the true temperature of the air, and this before and after every expedition. The fractional parts of a degree on both the attached and detached thermometers were noted only by estimation, but written down to 10ths, being more convenient in the computation; for I may remark, that one-third of a degree on the attached thermometer is equal to about $\frac{1}{1000}$ inch on the barometer; this attention, therefore, to the sub-divisions of the degrees became necessary. I conclude, lastly, with presuming, that the weight [(k)] of any column of air may be measured with these barometers to ,008 inch, though all the errors should lye the same way.

Leaving Geneva about half past six in the morning, August 20th, I arrived at the place A of my base a little before eight; near to which there happened to be a shepherd's house, in which I left one of my barometers (N° 1.) with a servant, to examine and observe it every five or ten minutes for near nine hours successively,

by —,013 inch: it is difficult to account for this. May 10th, 1776, at Rome, N° 1. stood lowest by —,001. June 12th, at Naples, N° 1. stood lowest by —,008. Sept. 10th, in London, N° 1. stood highest by +,006. These apparent variations may possibly arise from some alteration in the frame-work of the barometers through moisture, &c.

(k) I must not be understood to mean, that the length of any column of air may be measured to an equal accuracy, even though our theory should be perfect: this will be the subject of inquiry in its proper place.

until

until I returned; the windows and doors of the room, in which the inſtrument was placed, being left open, by which means there was a free communication with the outward air, and the barometer not expoſed to the Sun. The detached thermometer was hung on the window towards the north-eaſt, where there was neither direct nor reflected heat from the Sun (l). The two barometers

(l) I have thought proper to mention this, as it is almoſt the only circumſtance wherein my method of obſerving differed from Mr. DE LUC's, whoſe thermometers (if I miſtake not) were hung always in the Sun, and probably for this reaſon, becauſe the column of the atmoſphere between the two barometers, whoſe mean heat is to be determined, is (if the Snn ſhine) all expoſed to the Sun. I have, however, always preferred hanging them in the ſhade, and I give the following reaſons: all ſpurious and local heat from reflection is more eaſily avoided; no concentrated and falſe heat is acquired by the mounting, and thence communicated to the tube, even though the ball ſhould be inſulated; and, finally, becauſe I ſuſpect the real temperature of the atmoſphere in the Sun and in the ſhade to be the ſame, or at leaſt inſenſibly different. This may be thought to be advancing too much; but, to be ſatisfied of the poſition, I made no leſs than four-ſcore obſervations with four different thermometers of very different mounting, hung alternately expoſed to the Sun's rays, and ſcreened from them by the ſhade of a tree, in an open plain at ſome diſtance from the town of Geneva. The reſult was, that my beſt thermometer, with the ball inſulated, differed only 2° in the different ſituations; the others, more or leſs, as they were more or leſs connected with the frames in which they hung. One of them, incloſed in a glaſs tube, roſe 12° higher than the true temperature, which was 77°. It ſhould ſeem then, that the variety in the mounting occaſioned this difference; and this effect of the materials, of which the inſtrument is made, cannot be wholly avoided, as the glaſs itſelf, which conſtitutes the ball of the thermometer, will acquire and contain more or leſs, in proportion to its thickneſs and opacity. If a thermometer were perfect, it would reflect all the rays that it receives. More might be added to corroborate this idea, but it would ſwell this note to an unwarrantable length.

were

were here compared; and at a quarter after nine, beginning my walk, I arrived, not without ſome fatigue, at the top of the mountain about noon. The view from thence was incredibly beautiful. Every object, that from Geneva was ſtriking, from thence appeared with an additional effect. The mountains ſeemed higher and nearer; the plain appeared a more perfect level, the ſmall inequalities from this height becoming inſenſible; and a larger portion of the lake preſented itſelf: behind me an innumerable collection of naked points and precipices, all new objects, that from below are hid by the mountain, afforded freſh and moſt aſtoniſhing ideas of this very ſingular part of the creation. The clouds however (for it was a little hazy) unfortunately prevented my ſeeing Mont Blanc and the Glacieres, which were ſtill farther behind. Some of the clouds were below me, and very near; exhibiting to me, at that time, a very ſingular phenomenon of the thunder grumbling under my feet. I was occupied here between four and five hours with different obſervations. The barometrical ones I am now going to relate; and I ſhall at the ſame time give the computations of them according to Mr. DE LUC's method, or rather according to Dr. HORSLEY's reduction of it to the ſcales and meaſures of this country (*vide* Philoſ. Tranſ. vol. LXIV.) with this difference, that I have

have reckoned the equation for the expanſion of quickſilver =,00323 inch for every degree of FAHRENHEIT'S thermometer in a column of 30 inches, inſtead of ,00312 which Mr. DE LUC uſed; the former I had collected from ſome of my own experiments made at Oxford in the beginning of the year 1773: this difference will not, however, occaſion an alteration in the reſult of any one of my obſervations of more than five inches, and may therefore be conſidered as of no account. Of the real value of this correction I ſhall ſpeak more hereafter.

The barometer was ſet up on the mountain at one o'clock, and left an hour and a quarter to acquire the temperature of the tent in which it was placed, before the firſt regular ſeries of obſervation was taken. The ſucceeding obſervations were made at intervals of near an hour each. I have ventured to ſet down the height of the barometer to ,0001 inch; but this is only the mean from three or four readings off. It ſeems that the heat of the tent was conſiderably greater than that of the external air; this, however, can only influence the expanſion of the quickſilver, ſhewn by the attached thermometer, and not the preſſure of the atmoſphere. Laſtly, the true difference in the height of the reſervoirs of the two barometers, by compariſon with A and C, was found equal 2831.3 feet geometrically.

Com-

Comparison of the first series.

Observations at the top of the mountain at C.

	Barom. N° 2. above at C. In. Pts.		Therm. attached.	Therm. detached.
	25.7120		78°.0	65°.0
Correct for the diff. of the 2 attached therm. 5°.9,	— 162			
Barometer at the top,	25.6958	Log.	4098621	
——— below,	28.3951	Log.	4532434	
Difference, or fall of the quicksilver,	2.6993	Diff. of Log.	433.813	the height in English fathoms,
Correct for 29°.7 of heat,	—	—	+ 28.728	
Correct height in fathom,	—	—	462.541 × 6	
Height in English feet by the barometer,		—	2775.246	
Height by the trig. method,	—	—	2831.3	
Difference, or error of the barometer $\frac{198}{10000}$,			— 56.1	

Observations below at A.

	Barom. N° 1. below at A. In. Pts.	Therm. attached.	Therm. detached.
	28.3990	72°.1	73°.9
Correct for the diff. of the barometer,	— 39		65.0 heat at C.
	28.3951		69.4 mean heat of the air.
			39.7 stand. temp. according to Dr. HORSLEY.
			+ 29.7 difference.

A detached thermometer in the tent stood at 72°.

Comparifon of the fecond Series.

Obfervation at the top of the mount at C.

	Barom. N° 2. above at C. In. Pts.	Therm. attached.	Therm. detached.
	27.7025	73°.4	64°.0
Correct for the Diff. of the two attached therm.	— 50		
Barometer at the top,	25.6975	Log. 4098908	
——— below, —	28.3901	Log. 4531669	
Difference, or fall of the quickfilver, —	2.6926	Diff. of Log. 432.751	{ approx. height in English fathoms.
Correct for 28°.8 of heat, — —		+ 27.787	
Corrected height in fathoms, — —		460.538 × 6	
Height in feet by the barometer —		2763.228	
——— by the trig. method, —		2831.3	
Difference, or error of the barometer $\frac{240}{10000}$,		— 68.1	

Obfervation below at A.

	Barom. N° 1. below at A. In. Pts.	Therm. attached.	Therm. detached.
	28.3940	71°.6	73°.0
Correct for the diff. of barometer,	— 39		64.0 heat at C.
	28.3901		68.5 mean heat.
			39.7 ftandard temperature.
			+ 28.8 difference.

A detached thermometer in the tent ftood at 69°.

During thefe obfervations the wind was S.W.; the weather hazy, accompanied with a little thunder.

Com-

Comparison of the third series.

Observations at the top near c.

	Barom. N° 2. above at c. In. Pts.		Therm. attached.	Therm. detached.
	25.6900		69°.7	62°.0
Correct for the diff. of the 2 attached therm. 1°.4,	+ 38			
Barometer at the top,	25.6938	Log.	4098283	
——— below, —	28.3896	Log.	4531593	
Difference, or fall of the quickfilver, —	2.6958	Diff. of Log.	433.310	Approx. height in fathoms.
Correct for 27°.5 of heat,	—	—	+ 26.582	
Correct height in fathoms,	—	—	459.892 × 6	
Height in feet by the barometer,		—	2759.352	
——— by the trig. method,	—	—	2831.3	
Difference, or error of the barometer $\frac{254}{10000}$,			— 71.9	

Observations below near to A.

	Barom. N° 1. below at A. In. Pts.	Therm. attached.	Therm. detached.	
	28.3935	71°.1.	72°.5	
Correct for the diff. of barometer,	— 39		62.0	heat at c.
	28.3896		67.2	mean heat.
			39.7	ftandard temperature.
			+ 27.5	difference.

A detached thermometer in the tent ftood at 65°.

These

Theſe obſervations then ſeem to prove that the barometrical rules were a little defective as to the true ratio between the gravities of air and quickſilver, *viz.* in the value of an inch of quickſilver in the torricellian tube, expreſſed in inches of the atmoſphere with a given temperature. The firſt compariſon gives for this error in defect −19.8 feet in every 1000 feet; the ſecond, 24.0 feet; and the laſt, 25.4 feet: the mean of the three is 23.1 feet; and by ſo much we may conclude that theſe rules, in greater heights alſo, will give the difference of elevation too little, *viz.* by $\frac{1}{43}$ nearly[m]. But it will be fair to make the experiment.

(*m*) Leſt any ſuſpicion ſhould ariſe of a diſagreement between the actual meaſures taken by Mr. DE LUC and myſelf, I may obſerve, that the mean reſult of three obſervations, which I made independently of each other on the height of the Pitton or point C above the lake of Geneva, agree with the mean reſult of Mr. DE LUC's operation from the levelling and the quadrant, to leſs than twelve inches; a greater correſpondency than which cannot be expected: and this was the true reaſon why I choſe the ſame ſpot he had pitched upon. "*Le rocher* "*iſolé, qui domine toute la montagne.*" As a further confirmation, I compared his ſtandard ſteel rod of twelve Paris inches, which his brother obligingly furniſhed me with, with my braſs one, and found twelve inches on Mr. DE LUC's rule

was on my rule, with 71° of heat, — —	12.784 Eng. inches.
Correction for the difference of expanſion between braſs and ſteel with 16° of heat, — —	+ 07
Length of Mr. DE LUC's French foot with 55°, —	12.7847
True length of the French foot (*vide* Phil. Tranſ.)	12.7890
Error or difference from the true Paris foot —	—,0043 = $\frac{1}{3000}$ nearly.

The Mole is a convenient, insulated mountain, situated about eighteen miles east of Geneva, and rising near five thousand feet above the lake, much higher than any body, that I know of, has ever made these experiments at, with the required precision. On this summit I determined to confirm or correct my discovery, and communicated my intentions to Mr. DE SAUSSURE, a very ingenious gentleman of this place, and well skilled in various parts of natural and experimental philosophy, who gave me all the information necessary, and obligingly promised to accompany me, as did also Mr. TREMBLEY, assistant to Mr. MALLET, well known in the astronomical world. This expedition was undertaken in the latter end of August and beginning of September. I shall here beg leave to set the reader down at the bottom of the mountain, and flatter myself he will accompany me to the top. It was about five in the afternoon when we left St. Joire, a wretched little village at the foot of the mountain to the east, and where we had dined in a most miserable *auberge*, preparing to ascend the summit on foot, being seven or eight in company, including guides and servants, who carried my instruments, provisions, &c.; the former consisting of the equatorial, the barometer, different thermometers, electrical balls, an hygrometer, and a dipping-needle; together with another barometer

of Mr. DE LUC'S conftruction, a variation-needle, a level belonging to Mr. DE SAUSSURE, and a tent. Thus accoutered we proceeded up an afcent, not however very fteep, for three hours and a half without intermiffion, the path leading in a fpiral kind of direction, very rugged and full of loofe pieces of rock that are brought down with the melting fnows, paffing through romantic woods of fine firs and other trees, interfperfed here and there with a thin foil of excellent pafture. Before we arrived at the hut, where we were to fleep (for our intention was to lay upon the mountain that night, in order to have the more time the next morning for our operations) having walked on a little too far before, we loft fight of our guides. We called feveral times, but were never anfwered:—the night was now coming on; a kind of fog appeared, with fmall rain; our fituation became fomewhat embarraffing. We called again, but were anfwered by nothing but an echo, the place being a moft profound folitude. We began now to confider ourfelves as loft. Mr. DE SAUSSURE, though he had been feven or eight times before upon the mountain, found himfelf in doubt concerning the way; but after a fhort dilemma thought it beft to proceed. We did; and now began to perceive at a diftance fome little huts or hovels indiftinctly: a few more fteps affured us we were right, and about nine

o'clock

o'clock we had the good luck to find ourſelves at the very hovel, where we were to reſt that night. I own I now found myſelf quite contented, though I did not at all know what kind of place I was going to enter. It proved to be a little hut made of boards, conſiſting of one apartment only, eighteen or twenty feet ſquare, and about twelve high in the center, without any windows or chimney for the ſmoke, except what was made by the holes in the roof, and the interſtices between the boards at the ſides, which were rudely put together, ſcarce cloſer than park-palings, affording an equal entrance to the wind, rain, and ſnow; for as theſe hovels are inhabited only for about four months in the ſummer, they are conſtructed without the leaſt mortar or cement in the world; an humiliating witneſs this, how ſimple the architecture which nature and neceſſity ſuggeſt. On entering we found a comfortable fire, and the little *cabane* inhabited by a couple of Alpine ſhepherdeſſes and their two cows, on whoſe whey and ſome very coarſe bread they wholly ſubſiſted, not diſcontented but even proud of their lot; and who, out of a ſingular ſpecies of contempt, call the inhabitants of the plain *mange-rotis*, that is, eaters of roaſt-meat. Their language too was different; not French nor Italian, but partaking ſomething of both; or, as I have been ſince informed, a corruption of the ancient Celtic.

 A few

A few minutes after our arrival our guides rejoined us: it was now night, and in this rather too artless habitation we were obliged to lay in a little loft over the cows, our beds some leaves and clean hay, and my bolster my portmanteau[(n)]. I had had the caution to bring some sheets with me, and, being a little tired with my walking, slept five hours pretty soundly, though much starved, having no other curtains than what this wooden canopy afforded, through which the stars shone most brilliantly. Between four and five we arose; found the heavens beautifully serene, and, having eaten some of our provisions, left this habitation, which might be situated about two-thirds of the way up the mountain; and beginning our march about half after five reached the summit a quarter before seven; but not without a good deal of climbing, and sometimes up an ascent of near 40° for several hundred feet. One of my servants, before he got half way, found his head turn round, and himself so giddy, at the height and precipices (a frequent effect in these sort of places) that he was obliged to return to the hut. In the ascent I saw the Sun rising behind one of the neigh-

(n) ——————— Frigida parvas
Præberet spelunca domos, ignemque, laremque,
Et pecus, et dominos communi clauderet umbrâ;
Sylvestrem montana torum cùm sterneret uxor
Frondibus et culmo. JUV. Sat. vi.

bouring

bouring alps with a moſt beautiful effect, and the ſhadow of the mountain we were then upon extended fifteen or twenty miles weſt. We had now reached the ſummit; and there my curioſity finiſhed in aſtoniſhment. I perceived myſelf elevated 6000 feet in the atmoſphere, and ſtanding as it were on a knife-edge, for ſuch is the figure of the ridge or top of this mountain; length without breadth, or the leaſt appearance of a plain, as I had expected to find. Before me an immediate precipice, *à pic*, of above 1000 feet, and behind me the very ſteep aſcent I had juſt now mounted. I was imprudently the firſt of the company: the ſurprize was perfect horror, and two ſteps further had ſent me headlong from the rock.

On this ſpot, with ſome difficulty, we fixed the inſtruments, and commenced our operations, after ſome time ſpent in admiration at the proſpect, and familiarizing myſelf to the ſcene. Before me, at ſome diſtance, was ſpread the plain in which lay Geneva and the lake; behind it roſe the Dole, and the long chain of Mont Jura as far as the fort La Cluſe, which we entirely commanded, as well as ſome of the country beyond it. A little to the left, and much nearer, lay Mont Saleve, which from this height appeared an inconſiderable hill: to the right and left nothing but immenſe mountains, and pointed rocks of every poſſible ſhape, and forming tremendous precipices. In the

vale beneath, ſeveral little hamlets, and the moſt beautiful paſturages, with the river Arve winding and ſoftening the ſcene; from whence aroſe a thick evaporation, collecting itſelf into clouds, which on the lake, that was quite covered with them, had the appearance of a ſea of cotton, the Sun-beams playing in the upper ſurface of them with thoſe tints that are ſeen in a fine evening. To the ſouth-weſt appeared the lake of Annecy; behind us, taking up one-fifth of our horizon, lay the Glacieres, and amongſt them, towering above all the reſt, ſtood Mont Blanc. The circumference of the horizon might be about 200 Engliſh miles; and, though not one of the moſt extenſive, yet certainly one of the moſt varied in the world. From this ſpot the clouds had a ſtriking appearance to an inhabitant of the plain; very few of them at above one-fifth of the height that we were now at; not governed by the wind, but moving in every poſſible direction; ſome of them ſeemed creeping along the ground, whilſt others were riſing perpendicularly between the hills. And I may here remark, that from Geneva I have obſerved the clouds were generally three days in the week below the ſummit of Mont Saleve; ſo that the ordinary region of theſe vapours ſeems to be at that height in the atmoſphere, where the barometer would ſtand at about 26 inches in this climate.

While at the top of the Mole, I was very ſenſible of the cold, there being a briſk wind, which, though ſouth, came over the mountains of ice, and was very keen; inſomuch that, about two hours after I had been there, I nearly loſt the uſe of my fingers, and found my lips much affected and parched from the tranſition, having been a good deal heated in aſcending with two waiſtcoats and a great coat on. The thermometer, however, when I firſt mounted, ſtood no lower than 48°. I muſt here aſk pardon for this long digreſſion, which I have ventured to tranſcribe from my journal written upon the ſpot.

To return then to the obſervations. After what has been ſaid reſpecting thoſe on Mont Saleve, it will ſuffice here to mention, that by repeated meaſurements I determined the horizontal length of the baſe 1, 2 (ſee the chart) to be $=1250$ ft. 3.9 inch; the $\angle$ at $1=95° 37' 28''$; $\angle$ at $2=77° 48' 53''$; and the $\angle$ at $3=6° 33' 49''$. The mean corrected angle of elevation of 3 from 1 $=21° 29' 34''$; ditto of 3 from $2=21° 3' 41''$; and laſtly, the elevation of 2 from $1=0° 47' 24''$.

		Feet.
Theſe obſervations give for the length of the ſide 1, 3,	—	10691.9
— — — — 2, 3,	—	10886.7
Height of 3 above 1, — —	—	4212.8
——— 3 above 2, — — —	—	4194.8
——— 2 above 1, — — —	—	17.2
And conſequently, 3 above 1 deduced from the obſervation at 2,	—	4212.0
And laſtly, the mean height of 3 above 1 from the determination at each end of the baſe, — —	—	4212.4

The difference in height, however, between the two barometers was only 4211.3 feet.

Here follow the barometrical obſervations (o), and their reduction.

(o) Made between the hours of eight and twelve, in the open air and not in the tent, which could not be pitched on accout of the ſmallneſs of the plain at the ſummit; a briſk ſouth wind, but fair. The barometer was ſcreened by an umbrella.

Com-

Comparison of the first series on the Mole.

Observation at the top at 3.

	Barom. N° 2. above at c. In. Pts.	Therm. attached.	Therm. detached.
	24.1437	57°.0	54°.8
Correct for the Diff. of the two attached therm. 3°.4,	+ 88		
Barometer at the top,	24.1525	Log.	3829621
——— below, —	28.1253	Log.	4490971
Difference, or fall of the quickfilver, —	3.9728	Diff. of Log.	661.350 { approx. height in fathoms.
Correct for 18°.6 of heat, — —			+ 27.431
Corrected height in fathoms, — —			688.781
			× 6
Height in feet by the barometer —			4132.686
——— by the geometrical measurement,			4211.3
Difference, or error of the barometer,			— 78.6 = $\frac{187}{10000}$.

Observation below at 1.

	Barom. N° 1. below at 1. In. Pts.	Therm. attached.	Therm. detached.
	28.1295	60°.4	61°.9
Correct for the diff. of barometer,	— 42		54.8 heat at 3.
	28.1253		58.3 mean heat.
			39.7 standard temperature.
			+ 18.6 difference.

Com-

Comparison of the second Series.

Observation at the top at 3.

	Barom. N° 2. above at 3. In. Pts.	Therm. attached.	Therm. detached.
	24.1420	56°.9	56°.0
Correct for the diff. of the two attached therm. 3°.5,	+ 91		
	24.1511	Log. 3829369	
	28.1258	Log. 4491049	

Difference, or fall of the quicksilver, —	3.9747	Diff. of Log. 661.680	{ approx. height in fathoms.
Correct for 19°.2 of heat, — —		+ 28.330	
Correct height in fathoms, — —		690.010 × 6	
Height in feet by the barometer, —		4140.06	
—— by the geom. method, —		4211.3	
Difference, or error of the barometer,		— 71.2 = $\frac{169}{10000}$.	

Observation below at 1.

	Barom. N° 1. below at 1. In. Pts.	Therm. attached.	Therm. detached.
	28.1300	60°.4	61°.8
Correct for the diff. of barometer,	— 42		56.0 heat at 3.
	28.1258		58.9 mean heat.
			39.7 standard temperature.
			— 19.2 difference.

Com-

Comparison of the third Series.

Observation at the top at 3.

	Barom. N° 2. above at 3. In. Pts.		Therm. attached.	Therm. detached.	
	24.1670		$56^{\circ}.0$	$56^{\circ}.0$	
Correct for the diff. of the two attached therm. $4^{\circ}9$.	+ 127			☉ 57.5 (*p*)	
	24.1797	Log.	3834509		
	28.1278	Log.	4491358		
Difference, or fall of the quickfilver,	3.9481	Diff. of Log.	656.849		Approx. height in fathoms.
Correct for $19^{\circ}8$ of heat,	—	—	+ 29.0		
Correct height in fathoms,	—	—	685.849		
			× 6		
Height in feet by the barometer,	—		4115.094		
——— by the geom. method,	—		4211.3		
Difference, or error of the barometer,			$- 96.2 = \frac{228}{10000}$.		

Observation below at 1.

	Barom. N° 2. below at 2. In. Pts.	Therm. attached.	Therm. detached.
	28.1320	$60^{\circ}.9$	$63^{\circ}.0$
Correct for the diff. of the barometer,	— 42		56.0 heat at 3.
	28.1278		59.5 mean heat.
			39.7 standard temperature.
			19.8 difference.

(*p*) In this column for the detached thermometer at the top of the mountain, in this and the following observations, are inserted two numbers, the upper one expressing the heat in the shade; and the lower one, with this mark ☉ prefixed, the heat in the Sun. The computation, however, is made from the former; this may serve to shew the difference.

Com-

Compariſon of the fourth ſeries.

Obſervation at the top at 3.

	Barom. N° 2. above at 3. In. Pts.		Therm. attached.	Therm. detached.
	24.1780		57°.2	56°.0
Correct for the diff. of the two attached therm. 4°6,	+ 119			☉ 57.5
	24.1899	Log.	3836341	
	28.1318	Log.	4491976	
Difference, or fall of the quickſilver,	3.9419	Diff. of Log.	655.635	approx. height in fathoms.
Correct for 20°.3 of heat,	—	—	+ 29 678	
Correct height in fathoms,	—	—	685.313 × 6	
Height in feet by the barometer,	—		4111.878	
——— by the geom. method,	—		4211.3	
Difference, or error of the barometer,			— 99.4 = $\frac{235}{10000}$.	

Obſervation below at 1.

	Barom. N° 1. below at 1. In. Pts.	Therm. attached.	Therm. detached.
	28.1360	61°.8	63°.9
Correct for the diff. of the barometer,	42		56.0 heat at 3.
			60.0 mean heat.
	28.1318		39.7 ſtandard temperature.
			+ 20.3 difference.

Com-

Comparison of the fifth series.

Observations at the top at 3.

	Barom. N° 2. above at 3. In. Pts.	Therm. attached.	Therm. detached.
	24.1840	59°.6	57°.0
			⊙ 59.3
Correct for the diff. of the 2 attached therm. 2°.8,	+ 73		
	24.1913	Log.	3836592
	28.1308	Log.	4491820

Difference, or fall of the quickfilver,	3.9395	Diff. of Log.	655.228 approx. height in fathoms,
Correct for 20°.8 of heat,	—	—	+ 30.391
Correct height in fathom,	—	—	686.619
			× 6
Height in feet by the barometer,		—	4113.714
———— by the geom. method,		—	4211.3
Difference, or error of the barometer,			— 97.6 = $\frac{231}{10000}$.

Observations below at 1.

	Barom. N° 1. below at 1. In. Pts.	Therm. attached.	Therm. detached.
	28.1350	62°.4	64°.0
Correct for the diff. of the barometer,	— 42		57.0 heat at 3.
			60.3 mean heat.
	28.1308		39.7 ftandard temperature.
			+ 20.8 difference.

Com-

Comparifon of the fixth feries.

Obfervation at the top at 3.

	Barom. N° 2. above at 3. In. Pts.	Therm. attached.	Therm. detached.
	24.1900	61°.0	57°.0
Correct for the diff. of the two attached therm. 1°6,	+ 41		⊙ 60.0
	24.1941	Log. 3837095	
	28.1268	Log. 4491204	

Difference, or fall of the quickfilver,	3.9327 Diff. of Log.	654.157 { approx. height in fathoms.
Correction for 20°6 of heat,	— —	+ 30.048
Correct height in fathoms,	— —	684.157 × 6
Height in feet by the barometer,	—	4104.942
———— by the geom. method	—	4211.3
Difference, or error of the barometer,		—106.4 = $\frac{252}{10000}$.

Obfervation below at 1.

	Barom. N° 1. below at 1. In. Pts.	Therm. attached.	Therm. detached.
	28.1310	62°.6	63°.6
Correct for the diff. of the barometer,	— 42		57.0 heat at 3.
	28.1268		60 3 mean heat.
			39 7 ftandard temperature.
			20.6 difference.

To

To collect these last experiments in one point of view.

				Feet.
The 1st series gives for the error on every 1000 ft.				18.7
2d,	—	—	—	16.9
3d,	—	—	—	22.8
4th,	—	—	—	23.5
5th,	—	—	—	23.1
6th,	—	—	—	25.2
	The mean error,		—	21.7

which agrees within two feet in a thousand with the determination on Mont Saleve. This result then justifies my conclusion (in p. 556.) and proves that either the proportional gravity of air and quicksilver is now different from what it was, when M. DE LUC made his experiments, *viz.* from 1756 to 1760; or that his or my observations are defective. That my trigonometrical measurements were sufficiently exact, *viz.* to within two or three feet, I think I have already shewn; and even that his were also. Within what limits my barometrical errors are to be found is not difficult to determine from what has been before premised. That the scale of Mr. DE LUC's barometer was less accurate than mine, is, I think, without a doubt; and indeed he never attempted a division less than $\frac{1}{16}$th of a French line, or about $\frac{5}{1000}$

 of

of an inch Engliſh: and yet when I conſider the number of his obſervations, and the unexampled diligence and care with which he made them, I am obliged to attribute the difference of our reſults to ſome other cauſe than that of inaccuracy. If then future experience ſhould demonſtrate, that the denſity of the atmoſphere with a given heat is invariable, or nearly ſo; while the preſſure of a whole column of it continues the ſame, we may perhaps ſearch for the cauſe of our diſagreement from hence, *viz.* the barometers of Mr. DE LUC were not ſufficiently near each other in an horizontal direction: mine were ſeparated from two to three miles; and his, I believe, at double or triple that diſtance. It may be ſuſpected, I am well aware, that the ſyphon conſtruction of Mr. DE LUC'S barometer might occaſion this difference: let us ſee whether this be the caſe. Mr. DE SAUSSURE (whoſe inſtrument was of Mr. DE LUC'S conſtruction, and made, as I underſtood, under his inſpection) obſerved at the top of the Mole, or at leaſt nearly on the ſame level with my barometer, as follows:

And

	Barometer In.	L. 16ths.	Therm attached. DE LUC's scale.	Therm. det. REAUM. scale.
	22	8 0	+1°+	+10°−
And in English measure and FAHRENHEIT's scale, — —	24.1570		56	54.2
Mr. DE SAUSSURE's barometer ordinarily stands higher than mine N° 2. by (q),	−.0117			
Correct for the diff. of our attached therm. 1°,	+ 26			
Mr. DE SAUSSURE's barometer corrected,	24.1479			
My barometer N° 2. see the first series,	24 1437−		57	54.8
Difference, — —	+.0042 wholly inconsiderable.			

Our barometers may therefore be said to have agreed exactly.

Mr. DE SAUSSURE made a second comparison just before we left the top of the mountain, which proved as follows.

	Barometer In.	L. 16ths.	Therm. attached. DE LUC's scale.	Therm. detached.
	22	8 8	+4°	$+11\frac{20}{3}$
Or reduced to English measure and scale,	24.2014		61.7	57.9
Mr. DE SAUSSURE's barometer stands higher than mine N° 2. —	−.0117			
Corr. for the diff. of our attached therm. 0°.7,	—.0018.			
Mr. DE SAUSSURE's barometer corrected,	24.1879			
My barometer N° 2. see the sixth series,	24.1900		61.0	57
Difference, — —	—0.0021			

So that, in the first comparison, his barometer at the top of the Mole stood higher than mine by +,004 inch; and in the last, lower by −,002; the mean is higher by

(q) This we found by comparisons at the bottom of the mountain.

+,001

+,001, equal to about 10 inches in deducing the height of the mountain, a quantity wholly to be neglected. Finally, the mean of Mr. DE SAUSSURE's obſervations gives the defect of Mr. DE LUC's rules 21.9 in a thouſand. The conſtruction of the barometer had therefore no influence on this difference. But further, while Mr. DE SAUSSURE obſerved the height of the barometer on the Mole, Mr. DE LUC, the brother made a correſponding obſervation with a ſimilar inſtrument at Geneva. I ſhall relate this obſervation, computed after Mr. DE LUC's manner.

Mr.

	In.	L.	16ths.	16ths of a line.	Log.	Heat of the air. REAUM. Therm.	DE LUC's Therm.
Mr. DE SAUSSURE, at 4 feet below the summit of the Mole, —	22	8	0				
Mr. DE SAUSSURE's barom. stands higher than Mr. DE LUC's ordinarily by,		+	$1\frac{1}{2}$				
Thermometer attached +1°,		—	$0\frac{3}{4}$				
Correct height on the Mole,	22	8	$0\frac{3}{4}$	$=4352.\frac{3}{4}$	6387587	+10	$-15\frac{2}{3}$
Mr. DE LUC, 78 feet above the lake, —	27	0	0				
Therm. attahced +6°, —		—	6				
	26	11	10	=5178	7141620	+15	−4
Difference of the Log. — —					754.033	Sum	$-19\frac{2}{3}$
$19^{\circ}\frac{2}{3} \times \frac{750433}{1000}$ = the correction for the temperature,					−14.854		
Correct height in French toises, —					739.179 × 6		
Height in French feet, — —					4435.074		
Mr. DE LUC's barometer above the lake of Geneva,					+78.		
Mr. DE SAUSSURE's barometer below the summit of the Mole, — — —					+ 4.		
And consequently, the summit of the Mole above the lake, in French feet, — —					4517.		
Which reduced to English feet is, — —					4814.		
But, by a mean of my trigonometrical operations, this height is (*vide* chart) — —					4883.		
Difference, or error of the barometrical rules,					$-69. = \frac{14}{1000}$.		

This last observation serves at least to shew, that the error I am contending for is on the defective side, though it gives the quantity of it somewhat less, but by no means deserves that confidence which the other comparisons do; for, besides that this single observation may be concluded

less

leſs deciſive, the trigonometrical meaſurement is alſo leſs accurate from the diſtance; and, laſtly, to ſuppoſe the ſtate of the atmoſphere preciſely the ſame with reſpect to weight in two places twenty miles aſunder, is, I am afraid, a *poſtulatum* too hazardous to grant. I therefore ſay, that all theſe obſervations confirm the ſame truth, that the atmoſphere is lighter than Mr. DE LUC preſumed it. What had already been done may ſeem ſufficient for the eſtabliſhment of this fact; for I have always held, that a few obſervations, well made and faithfully related, do more in the interpretation of nature, than a multitude of crude, careleſs, and immethodical experiments. But I have not done: I wiſhed to put this matter out of all doubt, and accordingly undertook another expedition to the ſummit of Mont Saleve, on the 18th of September, and in a colder temperature: the experiments then made, with their reſults, were as follows:

The difference of actual height by the two barometers was 2828.9 feet, the barometer N° 1. ſtanding higher than N° 2. by +,0038 inch, when compared at the bottom of the mountain.

Com-

Comparison of the first series.

Observation at the top of the mountain.			Observation at the bottom.		
Barom. N° 2. at the top. In.	Therm. attached. °	Therm. detached. °	Barom. N° 1. below. In.	Therm. attached. °	Therm. detached. °
25.6533	58.0	56.2	28.4040	58.1	58.8

	Feet.
This gives for the height barometrically,	2755.6
But the true height was, —	2828.9
Difference, or error of the barometers,	$-73.3 = \frac{259}{10000}$.

Comparison of the second series.

Observation at the top of the mountain.			Observation at the bottom.		
Barom. N° 2. at the top. In.	Therm. attached. °	Therm. detached. °	Barom. N° 1. below. In.	Therm. attached. °	Therm. detached. °
25.6550	56.2	57.0	28.4040	58.5	60.8

	Feet.
This gives for the height barometrically,	2754.9
But the true height was, —	2828.9
Difference, or error of the barometers,	$-74.0 = \frac{262}{10000}$.

Compariſon of the third ſeries.

Obſervation at the top of the mountain.			Obſervation at the bottom.		
Barom. N° 2. at the top. In.	Therm. attached. °	Therm. detached. °	Barom. N° 1. below. In.	Therm. attached. °	Therm. detached. °
25.6620	56.2	57.2	28.4040	59.3	62.0

Feet.

This gives for the height barometrically, 2748.9
The height by the trigon. method was, 2828.9

Difference, or error of the barometers, $-80.0 = -\frac{282}{10000}$.

Compariſon of the fourth ſeries.

Obſervation at the top of the mountain.			Obſervation below.		
Barom. N° 2. at the top. In.	Therm. attached. °	Therm. detached. °	Barom. N° 1. below. In.	Therm. attached. °	Therm. detached. °
25.6600	56.4	57.4	28.4040	59.3	62.2

Feet.

This gives for the height barometrically, 2752.8
But the true height was, — 2828.9

Difference, or error of the barometers, $-76.1 = \frac{269}{10000}$.

In theſe compariſons I have not inſerted the whole of the computation, as that may eaſily be made by any perſon at leiſure. Finally, the mean of theſe four laſt ſeries

series gives for the error on 1000 feet, 26.8. I think I have now shewn, that the error actually exists; it remains that we determine precisely the quantity of it. For this purpose it will be proper to collect all the preceding observations in one point of view.

Table of the result of all the barometrical experiments.

Place of observation		True height trigonometrically.	Height by the barometers.	Mean heat.	Error in feet.	Error in 1000 feet.	Mean error in 1000 feet.
Mont Saleve,	1	2831.3	2775.2	69°.4	— 56.1	—19.8	—23.1
	2	——	2763.2	68.5	— 68.1	—24.0	
	3	——	2759 4	67.2	— 71.9	—25.4	
At the Mole,	1	4211.3	4132.7	58.3	— 78.6	—18.6	—21.7
	2	——	4140.1	58.9	— 71.2	—16.9	
	3	——	4115.1	59.5	— 96.2	—22.8	
	4	——	4111.9	60.0	— 99.4	—23.5	
	5	——	4113.7	60.5	— 97.6	—23.1	
	6	——	4104.9	60.3	106.1	—25.2	
Mont Saleve,	1	2828.9	2755.6	57.5	— 73.3	—25.9	—26.8
	2	——	2754.9	58.9	— 74.0	—26 2	
	3	——	2748.9	59.6	— 80.0	—28.2	
	4	——	2752.8	59.8	— 76.1	—26.9	
Mean of all, 23.6, and the temperature 61°.4.							
The Mole, from two observations of Mr. DE SAUSSURE, ——		4211.3	——	—	— 92.	—21.8	—16.2
The same by Mr. DE SAUSSURE, and Mr. DE LUC, at Geneva,		4883.	4814.	—	— 69.	—14.	
According to Mr. DE LUC's own observations, see *Recherches sur l'Atmosphere*,	the Mole,	4882.8	4860.	—	— 22.8	—. 47	
	the Dole,	4292.7	4210.	—	— 82.7	—19.5	
	the Buet,	8893.6	8770.	—	—123.7	—13.9	
	M^t Blanc,	14432.5	14093.	—	—339.5	—23.5	

The

The titles of the columns are ſufficiently clear to make a farther explanation of this table unneceſſary; and it appears, I think inconteſtably, upon taking a mean of my thirteen obſervations (and I ſhall here conſider only my own) on Mont Saleve and the Mole, that this error is about $23\frac{1}{2}$ feet on every thouſand; that is, the rules of Mr. DE LUC give the height by ſo much too little. At the bottom of the foregoing table I have ſubjoined ſix other compariſons, ſome of them from Mr. DE LUC's own obſervations, as recorded in his valuable work; which however I muſt add, are certainly of leſs authority in this inquiry, as they were made with barometers a great way diſtant from each other, *viz.* near thirty miles: beſides which, the geometrical heights are, for the ſame reaſon, not ſo accurately aſcertained. I have, however, ventured to make what uſe I could of them, *viz.* to ſhew that theſe two give a reſult on the ſame ſide, though not exactly the ſame; and to urge the neceſſity of a certain vicinity in thoſe obſervations from whence a theory is to be deduced.

Shall I be permitted to adduce another proof, in confirmation of what has been advanced? When I firſt took up the conſideration of meaſuring altitudes in the atmoſphere with the barometer, and had heard only of Mr. DE LUC's labours, it occurred to me, that there was a

6 much

much more ſimple method of arriving at this theory, than either he or I have ſince purſued. It was this; to determine hydroſtatically the ſpecific gravities of air[(r)] and quickſilver, with a given temperature and preſſure; the increaſe of volume, or change of gravity, with a given increaſe of heat being ſuppoſed to be known by the experiments of BOERHAAVE[(s)] and HAWKESBEE[(t)], which might be farther examined by ſimilar ones; and preſuming that the geometrical ratio in the air's denſity, as you advance upwards from the earth's ſurface, had been ſufficiently demonſtrated[(u)]. For the proportional gravity of quickſilver to air will expreſs inverſely the length of two equiponderant columns of theſe fluids, that is, when the columns are taken infinitely ſmall[(x)]. With theſe

(*r*) It may ſeem particular that I ſhould propoſe an experiment ſuppoſed to be very well known, and which hardly any elementary treatiſe on chemiſtry or experimental philoſophy will not furniſh us with an example of; the weight of a given quantity of air. BOYLE, HALLEY, HAWKESBEE, HALES, each of them have tried it, and many others ſince their time: but the misfortune is, all theſe experiments have been but groſs approximations, without due attention to the heat; and yet the determination of HAWKESBEE ſeems to have been followed by one-half of Europe in Pneumatical reſearches. Indeed I only know of one experiment that has the leaſt title to preciſion, and that is Mr. CAVENDISH's, briefly related in the LVIth volume of the Philoſophical Tranſactions.

(*s*) Elementa Chemiæ.

(*t*) Phyſico-mechanical Experiments.

(*u*) COTES's Hydroſtat. Lectures, *et alibi*.

(*x*) I am not ſorry to anticipate the reader's remark here, that this obſervation is not new; ſince I find that I have been treading the ſame ſteps with Mr.

theſe ideas I made the following experiment. I cauſed a glaſs veſſel to be blown ſomething like a Florence flaſk, or rather larger; to the neck of this was adapted a braſs cap with a valve opening outwards, and made to ſcrew on or off, together with a male ſcrew, by which it was fixed to an excellent pump of Mr. NAIRNE'S conſtruction, and exhauſted of its air, or at leaſt rarified to a known degree: the veſſel was then carefully weighed with a ſenſible balance, and again after the air was re-admitted; the difference gave the weight of the air that had been exhauſted. After having repeated this two or three times, the veſſel was exactly filled with water as far as the valve, which had been the term of capacity for the air; this was done by ſcrewing on the cap till the ſuperfluous water oozed all out, and upon inverting the veſſel there appeared not the leaſt ſign or bubble of air; I therefore concluded, that the volume of water was preciſely the ſame as had been the volume of air, a circumſtance that ſhould be accurately attended to. It was then carefully weighed, and compared with its weight when full and deprived of its air. It will readily be ſeen, that I had then the ſpecific gravity of the two fluids, upon ſuppoſition that the figure of the glaſs had not altered

Mr. BOYLE and Dr. HALLEY, who both made uſe of this method; the one with a view to determine the limits of the atmoſphere; and the other the height of Snowden.

by

by preſſure during the experiment; and this effect may be preſumed to have been the moſt ſenſible, when the veſſel was filled with water, the preſſure at that time being from within. To aſſure myſelf of this, I let in a ſmall quantity of air, which formed a bubble of about one-third of an inch in diameter, and upon immerging the glaſs in another veſſel of water, whereby the preſſure within was counterpoiſed by a preſſure without, the bubble ſeemed to contract itſelf by a quantity, as I found afterwards, equal to about two grains in weight, or $\frac{1}{8000}$ of the whole contents. I therefore concluded, that this correction was hardly worth taking notice of, and ſtill leſs the effect from external preſſure when the glaſs was exhauſted. At every operation the height of the barometer and thermometer (placed cloſe to the veſſel when the air was weighed) was noted down, together with the height of the pump-gage, which, compared with the barometer in the room, ſhewed the quantity exhauſted. The reſult of the experiment was as follows, the barometer in the room ſtanding at 29.27 inches, and the heat of the room 53°.

The

	Feet.
The bottle empty or exhauſted till the gage ſtood at 29.15 inches weighed (determined from four different trials, and the balance turning with $\frac{1}{15}$ of a grain) — — —	2657.40
Increaſe of weight when filled with air, from four trials certain to $\frac{1}{10}$ of a grain — — —	+16.13
Bottle filled with water, whoſe heat was 51°, — —	16220.00
Weight of the water, excluſive of the bottle, — —	13562.60

But the bottle was exhauſted only in the proportion of 29.15 inches to 29,27 inches; therefore if a perfect *vacuum* could have been made, the difference of weight would have been 16.22 grains inſtead of 16.13 grains. Again, the water was colder than the air by 2°; the one being 53°, and the other only 51°: now water, from former experiments, I find to expand about $\frac{3}{10000}$ with 2° of heat; therefore, if the water had been of the ſame temperature with the air that was examined, the weight of an equal volume would have been only 13558,5 grains; and laſtly, 13358.5 divided by 16.22 gives 836 (y), and by ſo much is water heavier than air in theſe circumſtances.

(y) HAWKESBEE's experiments made the air 850 lighter than water, the barometer being at 29 7; and Dr. HALLEY ſuppoſed it about 800. Mr. CAVENDISH, in weighing 50 grains of air, when the barometer was at 29$\frac{3}{4}$, and the thermometer at 50°, concluded the ſpecific gravity of air to be about 800 alſo. Now my experiment, reduced to the ſame circumſtances with his, would give 817 for this gravity, no great difference in an affair of ſuch delicacy.

By

By former experiments I find the specific gravity of the quick-silver of my barometers, compared with rain-water in 68° of heat, as, — — — —	13.606 to 1
And 68°—53°=15°, correct therefore for 15° of expansion of quicksilver, — — —	+.018
Correct for 15° of expansion of air, — —	—.031
True specific gravity of quicksilver, with 53° of heat,	13.594
Which multiplied by the specific gravity of air, —	×836
Gives for the comparative gravity of quicksilver and air, when the barometer is at 29.27, and the thermometer 53°, —	11364.6

	Feet.
And lastly, $\frac{1}{10}$th of an inch of quicksilver, when the barometer stands at 29.27 inches (*viz.* from 29.22 inches to 29.32 inches) with the temperature 53°, is equal to a column of the atmosphere of, —	94.7
This quantity, according to my barometrical observations, is, —	93.83
——————— to Mr. DE LUC's rules, — —	91.66

We see here then that the statical experiment agrees with the result of my barometrical ones to within about 11 inches in 100 feet, and I am not sure that it is not still capable of much farther precision; and though perhaps alone it might carry with it, to some persons, a less conclusive testimony, who reluctantly reason from the little to the great, yet, in conjunction with what has been before shewn, I think it has considerable weight; and I am the less inclined to reject such an indirect method of proof, as I have the great authorities of HALLEY and NEWTON on my side(z).

I have

(z) " Ce qu'il y a d'essentiel à observer ici," says Mr. DE LUC, " et vrai-" ment digne de remarque, c'est que par la seule connoissance des pesanteurs " specifiques de l'air et du mercure, HALLEY est parvenu à une regle très " approchante

I have thus endeavoured to ſhew then that the error of the theory is $-\frac{236}{10000}$ when the temperature of the air is 61°.4 (ſee the table of the reſult of the obſervations). It remains therefore, finally, that we deduce a rule, the error of which ſhall be nothing, *viz.* to find the temperature wherein the difference of the logarithms of the heights of the barometer, taken to four places of figures, will give the true difference of elevation in Engliſh fathoms. Previous to this inveſtigation, with which I intend to conclude this paper, it will be neceſſary to remark, that by repeated experiments with the barometer, I find a ſmall difference in the equation for the expanſion of air by a change of temperature, and even in that of quickſilver from the ſame cauſe, from what Mr. DE LUC's obſervations have given it[(a)]. I ſhall

" approchante de celle, qu'un grand nombre d'obſervations du baromètre dans " les Cordelières ont dicté depuis à M. BOUGUER: cependant malgré l'appui " que ces experiences ſe prêtent reciproquement, on verra qu'elles etoient encore " bien eloignées de fournir une regle generale." Recherches ſur l'Atmoſphere, ſect. 267.

(*a*) He indeed made his experiments on the atmoſphere itſelf with the barometer, in order to determine the variations of its denſity; but ſince it appears that the abſolute denſity of this fluid is different from what he ſuppoſed it, it is no bold conjecture to preſume that the degree of its variation ſhould be different alſo; and to aſcertain this point, I have preferred the inſtrument above-mentioned to the method uſed by Mr. DE LUC, how direct ſoever his may ſeem; for in determining minute quantities or equations, we muſt not embarraſs ourſelves with the compound effect of too many cauſes at a time.

not

not here trouble the reader with the experiments at large, too ſimple in themſelves to deſerve ſuch a detail, unleſs a future occaſion ſhould render that neceſſary, as the methods here uſed may be met with amongſt HAWKESBEE'S or Mr. BOYLE'S experiments; and content myſelf with relating only the reſult of the different trials.

1000 parts of air of the temperature of freezing and preſſure of 30½ inches, increaſed in volume by an addition of 1 degree of heat on FAHRENHEIT'S thermometer as follows:

Obſervations.		Number of degrees the air was heated.	Expanſion for 1° in 1000ths of the whole.	
With the firſt manometer,	1	14.6°	2.30	Mean from the firſt manometer 2.44.
	2	32.2	2.43	
	3	40.3	2 48	
	4	46.6	2.45	
	5	49.7	2.48	
	6	51.1	2.51	
	7	23.7	2.36	
	8	13.1	2.24	
With another manometer,	9	22.0	2.38	Mean from the ſecond manometer 2.42
	10	28.0	2.50	
	11	21.5	2.34	
	12	30.1	2.44	
	13	22.6	2.44	

 The

The mean of theſe two ſorts of obſervations, made with different inſtruments, is 2.43, *viz.* 1000 parts of the air at freezing become by expanſion from 1° of heat equal $\overset{\text{Pts.}}{1002.43}$ or $\overset{\text{Pts.}}{1002.385}$ with the ſtandard temperature 39°.7. Mr. DE LUC'S experiments reduced give this quantity equal $\overset{\text{Pts.}}{1002.23}$ (b) (ſee Tranſ.). It may be imagined, that I ſhould have had a more accurate concluſion by making theſe obſervations in greater differences of temperature than what is ſhewn in the ſecond column of the above table; but it did not appear ſo to me. On the other hand, I found that it was abſolutely neceſſary that the ſame heat ſhould be kept up for ſome hours together, in order that I might be ſure that the air within the inſtrument, the glaſs tube that contained it, and the air without it, all had acquired the ſame

(*b*) It has generally been ſuppoſed, that air expands $\frac{1}{400}$ with each degree of the thermometer, commencing from the mean temperature 55°; and, in conſequence of this, aſtronomers have computed tables for correcting their mean refractions; but, upon reducing the reſult of my obſervations to the temperature 55°, we ſhall have the correction of the refraction for 1° $= \frac{243}{105589}$ or $\frac{1}{435}$. Now according to Mr. DE LUC this equation is $\frac{223}{103345} = \frac{1}{463}$, which would produce a difference of about 4″ in the corrected refraction, upon an altitude of 5°, with the temperature 35°. If my numbers may be ſuppoſed to deſerve equal confidence, the error of the tables in common uſe, in the above circumſtances, would amount to only half that quantity, and therefore probably will be thought ſcarce worth correcting. I have mentioned this in order to obviate the concluſions that have been drawn by ſome perſons from Mr. DE LUC'S theory.

uniform

uniform temperature, which in my room I found not very eaſy to effect in heats greater than 70° or 80°. I have therefore preferred repeating the experiment with ſmall differences of heat; but ſuch, however, as will include almoſt all the temperatures in which barometrical obſervations are likely to be made, *viz.* from 32° to 83°.

It has been ſuſpected, in conſequence of ſome experiments made by a very ingenious member of this Society, that air does not expand uniformly with quickſilver; or that the degrees of heat ſhewn by a quickſilver-thermometer would be expreſſed on a manometer, or air-thermometer, by unequal ſpaces in a certain geometrical ratio. I do not deny this propoſition; but I have alſo very little reaſon to aſſent to it, if I may truſt my own experiments, which certainly evince that this ratio, if not truly arithmetical, is ſo nearly ſo as to occaſion no ſenſible error in the meaſuring of heights with the barometer; and that is all I contend for. The ſmall differences that are ſeen in the above table of this expanſion, deduced from a mean of 14° or of 40°, I would attribute rather to the errors of obſervation than to any actual irregularity in nature. If, however, this progreſſion be inſiſted upon, it ſhould ſeem, that the degree of the air's expanſion increaſes with an increaſe of heat; and that the difference of volume or denſity from 1° of

heat,

heat, any where within the limits above-mentioned, would be about one part in five thoufand from what I take it at a mean. I fhould not have infifted fo long on this circumftance, but in refpect to the known accuracy of the author of this hypothefis. Neither do I find any reafon to believe, that the expanfion of air varies with its denfity. I have tried air whofe denfity or preffure was equal to $23\frac{3}{4}$ inches, and alfo to forty inches; but the dilatation, with equal volumes and equal degrees of heat, was very nearly the fame in both cafes. I might add a great deal more on thefe manometrical experiments, but I am afraid it would be more tedious than ufeful. I proceed therefore to the expanfion of quickfilver.

This experiment was made with a tube, fomething like a thermometer, but confiderably larger than the ordinary fize, and open at one end; it was filled with quickfilver to a certain height, and then expofed to the temperatures of freezing and boiling repeatedly, the barometer being at 30 inches: the difference of the volume in each inftance was determined afterwards by accurately weighing the contents. I thus found, that if the quickfilver at freezing be fuppofed to be divided into 13119 parts, the increafe of volume by a heat of boiling water became equal to 208 of thefe parts $= \frac{10}{637}$, and $\frac{10}{637} \times \frac{1}{180} = \frac{1}{11466}$; and fuch would be the expanfion for

 each

each degree of the thermometer, commencing from the freezing point, $=0,00262$ inch on a column of 30 inches of the barometer, if the glafs had fuffered no expanfion during the experiment. This, however, has been found to be with 180° of heat $= \frac{1}{400}$ in folidity (*viz.* the cube of its longitudinal expanfion) and $\frac{1}{400} \times \frac{1}{180} = \frac{1}{72000} = 0,00042$ inch, for the effect of the expanfion of the glafs for 1° upon a column of 30 inches; this added to the quantity before found, which was only the excefs of the greater expanfion above the lefs, gives for the true equation for each degree 0,00304 inch when the barometer ftands at 30 inches(c). Mr. DE LUC's correction in this cafe was 0,00312; a difference fo fmall that I fhall take no notice of it as to its influence upon our obfervations. It may deferve a remark here, that this equation rigoroufly taken is variable according to the height of the thermometer; for 1°, which at

(c) It has been fufpected, and I believe will appear from very good obfervations, which however I never made myfelf, that the expanfion of quickfilver in the barometer is not directly as the heat fhewn by the thermometer, but in a ratio fomething different, owing to fome of the quickfilver being converted into an elaftic vapour in the *vacuum* that takes place at the top of the Torricellian tube, which preffes upon the column of quickfilver, and thus counteracts in a fmall degree the expanfion from heat. It does not, however, appear to be a confiderable quantity, not amounting to above one-fixteenth of the whole expanfion in a range of 40° of temperature; I fhall therefore venture to confider this equation as truly uniform, fince the error on ten thoufand feet would not amount to five.

freezing

freezing is $=\frac{1}{9891}$ of the whole volume, at the temperature 82° becomes $\frac{1}{9941}$, a difference indeed that may fairly be neglected, and which I neglect myſelf; yet I cannot help obſerving, in juſtice to Mr. DE LUC, that his method of reducing his barometers always to the ſame ſtandard temperature, was free from the error I am ſpeaking of.

To conclude, the defect of Mr. DE LUC's rules being ſuppoſed $\frac{236}{10000}$, or, which comes to the ſame thing, the correction being $+\frac{2417}{100000}$, when the temperature of the air is 61°.4, and the true expanſion of the air for each degree being $\frac{239}{100000}$ when the heat is 39°.7; required to find the temperature wherein the difference of the logarithms ſhall give the true height in Engliſh fathoms, that temperature, according to Mr. DE LUC, being 39°.74, and the expanſion $\frac{223}{100000}$.

Let T be the temperature 61°.4; S Mr. DE LUC's ſtandard temperature; E the expanſion for 1°; e the ſame, according to Mr. DE LUC; α the ſuppoſed correction of the rules, and x the temperature ſought. We have then the following formula, $\overline{\text{T}-\text{S}}\times\overline{\text{E}-e}$ [d] $-\alpha=\text{S}-x$, wherein proceeding with the above numbers $\text{S}-x$ comes out

(*d*) This ſign is negative, becauſe the aſſumed expanſion e is leſs than the true one E, and conſequently tended to increaſe the apparent error of the rules; had it been greater, α would have been +.

8°.50

8°.50, and consequently $x = 31°.24$ the temperature required; which, if it should be thought convenient, may be considered as the freezing point.

In the whole of the above inquiry I have taken no notice of the effect of gravity upon the particles of the air at different distances from the earth's center, which should doubtless enter into the account, and which would occasion the density of the atmosphere to decrease in a ratio something greater than the present theory admits of. In a height of four English miles Dr. HORSLEY finds (Phil. Transf. vol. LXIV.) that the diminution of density or volume from the accelerative force of gravity would be only $\frac{1}{500}$ part of the whole, or about 48 feet; and I may add to this, that this effect will be in the duplicate ratio of the heights, so that at one mile high it becomes only three feet. A like effect takes place also below the surface of the earth, as in measuring the depths of mines, &c. with this difference, that here it is but half the quantity; in the former instance gravity within the earth being simply as the distance from the center; they are both of them, however, circumstances that deserve no attention in practice.

This would be the place for me to enumerate the many desiderata, besides those already hinted at, that still remain for the perfection of this theory; such as the

laws of heat, that obtain in the different regions of the atmoſphere; the effects of moiſture, winds, the electric fluid, together with the weight and qualities of the air in different countries, &c.; that at the ſame time that I am congratulating the preſent age on one of the moſt brilliant diſcoveries in natural philoſophy, I may be underſtood alſo to encourage every lover of ſcience to ſtill farther enquiries in a branch of knowledge no leſs uſeful than ingenious; particularly in a kingdom wherein, from its commercial intereſts, and in conſequence its many inland navigations, every improvement in hydroſtatics, the art of levelling, or geometry, cannot but tend conſiderably to the public benefit. The ſources of ſcience are not eaſily exhauſted; multitudes of them remain wholly unexplored. If novelty can afford a charm, the path I am ſpeaking of, till of late, has been the leaſt frequented; witneſs the freſh, important truths in Pneumatical reſearches that, from zeal and faſhion, every day's experience affords. I might here offer too a tribute of applauſe (and am ſure in concert with this whole aſſembly) juſtly due to the indefatigable labours of him whoſe ſteps I have purſued; but I am convinced he will rather hear me acknowledge our obligations to the ancients than any panegyric of himſelf. Be the benefit we receive from them our encouragement to proceed.

Multum egerunt, qui ante nos fuerunt, ſed non peregerunt: multum adhuc reſtat operis, multumque reſtabit; nec ulli nato poſt mille ſæcula præcludetur occaſio aliquid adhuc adjiciendi." SEN. Epiſt. 64.

PART II.

IN the ſubſequent pages, which I have now the honour of laying before the Royal Society, I have drawn up, and I hope in a form the moſt commodious, the neceſſary tables and precepts for calculating any acceſſible heights or depths from barometrical obſervations, and without which I thought the preceding memoir would be incomplete; referring, however, to that for the proofs or elements from whence the tables have been computed. And herein I have endeavoured to adapt myſelf to the capacity of ſuch perſons as are but little converſant with mathematical computations, but who may have frequent opportunities of contributing ſomething to the advancement of ſcience by experiments with this uſeful

inſtrument, which is now become nearly in as common poſſeſſion as a pocket watch. I have induſtriouſly avoided the method of logarithms, propoſed by Dr. HALLEY, and adopted by Mr. DE LUC, both becauſe ſuch tables are not in the hands of every body, and becauſe I have perceived that many perſons of a philoſophical turn, though ſkilled only in common arithmetic, have been frightened by the very name: a method leſs popular, however elegant, would have been leſs generally uſeful. To theſe tables is ſubjoined a liſt of ſeveral altitudes, as determined by the barometer: this will ſerve to ſhew the uſe I have made of the inſtrument, and will at the ſame time exhibit the level of a great number of places in France, Savoy, and Italy, and, as I think, be no improper ſupplement to exemplify the rules. It might have been expected that I ſhould have ſaid ſomething on the theory of barometrical obſervations, and have laid down the laws and principles on which it depends; but as that has been ſo amply done by other writers of incontеſted authority, I ſhall content myſelf with inſerting only the following propoſitions.

1ſt, The difference of elevation of two places may be determined by the weight of the vertical column of the atmoſphere intercepted between them.

2d, If then the weight of the whole atmosphere at each place can be ascertained, the weight of this column, *viz.* their difference, will be known.

3d, But the height of the quicksilver in the barometer expresses the total weight of the atmosphere in the place of observation; the difference, therefore, of the height of the barometer, observed in two places at the same time, will express the difference of elevation of the two places.

4th, But further, the weight of this column of the atmosphere is liable to some variations, being diminished by heat, and augmented by cold; and again, a similar alteration takes place in the column of quicksilver, which is the measure of this weight.

5th, If then the degree of these variations can be determined, and the temperature of the air and quicksilver at the time of observation be known, the weight of this column of air, or the difference of elevation of the two places, will be concluded as certainly as if the gravity of these two fluids, with all heats, remained invariably the same: this is the whole mystery of barometrical measurement.

APPLICATION.

The height of the barometer in Engliſh inches at any two places at the ſame inſtant, and the heat (according to FAHRENHEIT'S thermometer) to which it is expoſed, being known, together with the temperature of the air at each place, obſerved with a ſimilar inſtrument; required the difference of elevation of the two places in Engliſh feet.

RULE.

Precept the 1ſt, With the difference of the two thermometers that give the heat of the barometer (and which, for diſtinction ſake, I ſhall call the attached thermometers[(e)]) enter table I. with the degrees of heat in the column on the left hand, and with the height of the barometer in inches, in the horizontal line at the top; in the common point of meeting of the two lines will be found the correction for the expanſion of the quickſilver

(*e*) It is ſcarce neceſſary to remark, that, in order to make good concluſive obſervations, it is proper to be furniſhed with two barometers, and four thermometers; *viz.* one attached or inſerted in the frame of each barometer; and the other two detached from them, in order to take the heat of the open air; for it will ſeldom be found, that the thermometer in the frame of the barometer and that in the air will ſtand at the ſame point, and for a very evident reaſon.

by

by heat, expreſſed in thouſandth parts of an Engliſh inch; which added to the coldeſt barometer, or ſubtracted from the hotteſt, will give the height of the two barometers, ſuch as would have obtained had both inſtruments been expoſed to the ſame temperature.

Precept the 2d, With theſe corrected heights of the barometers enter table II. and take out reſpectively the numbers correſponding to the neareſt tenth of an inch; and if the barometers, corrected as in the firſt precept, are found to ſtand at an even tenth, without any further fraction, the difference of theſe two tabular numbers (found by ſubtracting the leſs from the greater) will give the approximate height in Engliſh feet. But if, as will commonly happen, the correct height of the barometers ſhould not be at an even tenth, write out the difference for one entire tenth, found in the column adjoining, intitled *Differences*; and with this number enter table III. of proportional parts in the firſt vertical column to the left hand, or in the 11th column, and with the next decimal following the tenths of an inch in the height of the barometer (*viz.* the hundredths) enter the horizontal line at the top, the point of meeting will give a certain number of feet, which write down by itſelf; do the ſame by the next decimal figure in the height of the barometer (*viz.* the thouſandths of an inch) with this difference, ſtriking

ſtriking off the laſt cypher to the right hand for a fraction; add together the two numbers thus found in the table of proportionable parts, and their ſum ſubduct from the tabular numbers juſt found in table II.; the differences of the tabular numbers, ſo diminiſhed, will give the approximate height in Engliſh feet.

Precept the 3d, Add together the degrees of the two detached or air-thermometers, and divide their ſum by 2, the quotient will be an intermediate heat, and muſt be taken for the mean temperature of the vertical column of air intercepted between the two places of obſervation: if this temperature ſhould be $31^\circ\frac{1}{4}$ on the thermometer, then will the approximate height, before found, be the true height; but if not, take its difference from $31^\circ\frac{1}{4}$, and with this difference ſeek the correction in table IV. for the expanſion of air, with the number of degrees in the vertical column on the left hand, and the approximate height to the neareſt thouſand feet in the horizontal line at the top; for the hundred feet ſtrike off one cypher to the right hand; for the tens ſtrike off two; for the units three: the ſum of theſe ſeveral numbers added to the approximate height, if the temperature be greater than $31^\circ\frac{1}{4}$, ſubtracted if leſs, will give the correct height in Engliſh feet. An example or two will make this quite plain.

EXAMPLE

EXAMPLE I.

Let the height of the barometer, obſerved at the bottom of a mountain be 29.4 inches, the attached thermometer 50°, and the heat of the air 45°; at the ſame time that at the top of the mountain the barometer is found to ſtand at 25.190 inches, the attached thermometer at 46°, and the air-thermometer at 39°$\frac{1}{2}$; required the height of the mountain in Engliſh feet. Set the numbers down in the following order:

Obfervation at the bottom.

	Barometer.	Therm. attached.	Therm. in the air.
	In.		
	29.400	50°	45°
		46	
Diff. of the two attached thermometers,		4	

Obfervation at the top.

	Barom.	Therm. attached.	Therm. in the air.
	In.		
	25.190	46°	39°½
Correct for the diff. of the two attached therm. *viz.* 4°,	+ 10		45
			2)84½(42¼ mean heat.
Height of the uppermoft barometer, reduced to the fame heat as the lowermoft, *viz.* 50°, — —	25.200		31¼ ftandard heat.
			11 difference.

Correct for 11°, fee tab. IV.	
on 4000 feet	106.9
on 16 —	+ 5
or on 4016	+ 107.4

	In.	Feet.
Tabular number, fee tab. II. correfponding to, —	25.200 =	6225.0
The fame, correfponding to	29.400 =	2208.2
Approximate height in feet,		4016.8
Correction for 11° of heat on 4016 feet, add, — —		107.4
Correct height of the mountain. — — —		4124.2

Now the difference of the attached thermometer 50° and 46° is = 4°; and againft this number, in table I. in the column for 25 inches (being the height of the barometer in this cafe) I find 10, which added to 25.190, as this barometer was the coldeft, gives 25.200 inches for

I the

the height of the uppermoſt barometer reduced to the ſame heat as the lowermoſt: and in table II. oppoſite to 25.200 inches and 29.400 inches, I find reſpectively 6225.0 and 2208.2; their difference 4016.8 is the approximate height in feet. The degrees on the thermometer in the open air, $39°\frac{1}{2}$ and $45°$ being then added together, and afterwards divided by 2, give for the mean temperature of theſe obſervations $42°\frac{1}{4}$, or $11°$ above the ſtandard temperature, $31°\frac{1}{4}$: and laſtly, the correction for $11°$, in table IV. on 4000 feet I find = 106.9, and on 16 feet = 0.5; that is, 107.4 feet equal the whole correction, which added to 4016.8 gives 4124.2 feet for the correct height of the mountain.

EXAMPLE II.

Suppoſe the height of the barometer at the top of a rock had been obſerved at 24.178, the attached thermometer at $57°.2$, the air-thermometer at $56°$; the barometer below at 28.1318 inches, the attached thermometer $61°.8$, the detached one $63°.9$; what is the height of the rock?

Obſer-

Obſervation at the bottom.

	Barometer.	Therm. attached.	Therm. detached.
	In.		
	28.1318	61°.8	63°.9
		57.2	
Difference of the two attached thermometers,		4.6	

Obſervation at the top.

	Barom.	Therm. attached.	Therm. detached.
	In.		
	24.1780	57°.2	56°.0
Correct for the diff. of the two attached therm. *viz.* 4° 6, —	+0112		63.9
			2)119.9(59.95 mean heat.
			31.24 ſtandard temp.
Height of the uppermoſt barom. reduced to the ſame heat as the lowermoſt, namely 61°.8,	24.1892		28.71 difference.

			Feet.	Diff.
Tabular number, correſponding to,	24.1000		7388.0	107.9
The ſame, ſee tab. III.	800	86.0		
	90	9.7	—95 9	
	2	.2		
	24.1892		7292.1	
Tabular number, correſponding to,	28.1000		3386.6	92.6
The ſame, ſee tab. III.	300	28.0		
	10	0.9	—29.6	
	8	0.7		
	28.1318		3357.0	
And 3357.0 feet taken from —			7292.1	
Leaves the approximate height in feet,			3935.1	
Correction for 28°7 of heat on 3935 ft.			+274.3	
Correct height of this mountain,			4209.4	

Correct for 28°.7, ſee tab. IV.

28° on	3000 =	204.1
	900 =	61.2
	35 =	2.4
0.7 on	3000 =	5.1
	900 =	1.5
	35 =	0.0
28.7 on	3935	274 3

This

This laſt obſervation was actually made, and the height geometrically was determined to be 4211.3 feet, not quite two feet different. In this example it will be obſerved, that as the height of the barometer is ſet down to four places of decimals; the tabular numbers, anſwering to every tenth only, are corrected by means of table III. of proportional parts, for the remaining decimals 8, 9, and 2, in one place; and 3, 1, 8, in the other; and their ſum is ſubducted from the numbers found in table II. And laſtly, that in finding the correction for 28°.7 of heat, the fraction $\frac{7}{10}$ is conſidered as ſo many units, and another decimal is ſtruck off; thus the correction on 3000 feet for 7° is 51; but for $\frac{7}{10}$ it becomes 5. 1, and ſo of the reſt.

EXAMPLE III.

In the upper gallery of the dome of St. Peter's church at Rome, and 50 feet below the top of the croſs, I obſerved the barometer, from a mean of ſeveral obſervations, 29.5218; the thermometer attached being at 56°.6, and the detached one at 57°; at the ſame time that another, placed on the banks of the Tyber one foot above the ſurface of the water, ſtood at 30.0168, the attached thermometer at 60°.6, and the detached one at 60°.2; what was the total height of this building above the level of the river?

Obſer-

Obſervation below, at one foot above the Tyber.

	Barometer.	Therm. attached.	Therm. detached.
	In.	°	
	30.0168	60.6	60°.2
		56.6	
Difference of the two attached thermometers,		4.0	

Obſervation above, in the gallery of St. Peter's church.

	29.5218	56.6	57.0
Correct for the diff. of the two attached therm.	+ 120		60.2
			2)117.2(58.60 mean heat.
			31.24 ſtandard temp.
Height of the uppermoſt barom. reduced to the heat of the lowermoſt *viz.* 60.5, —	29.5338		27.36 difference.

			Feet.	Diff.
Tabular numbers correſponding to, —	29.5000		2119.7	88.2
	300	26.4		
	30	2.6	—29.7	
	8	7		
	29.5338		2090.0	
Tabular numbers correſponding to, —	30.0000		1681.7	86.7
	100	8.7		
	60	5.2	—14.6	
	8	7		
	30.0168		1667.1	
			2090.0	
Approximate height,			422.9	
Correction for 27°.4 of heat on 422 feet,			+28.0	
Difference of height of the barometers,			450.9	
Loweſt barom. ſtood 1 foot above the river,			+1.0	
Top of the croſs above the gallery was,			+50.0	
Total height of the top of the croſs above the river Tyber, —			501.9	
The ſame meaſured the ſame day geometrically was, — —			502.2	

Correction for 27°.4		
27° on	400 =	26.2
	22 =	1.4
0.4 on	400 =	.4
27.4 on	422 =	28.0

When

When the difference of the heights of the quickfilver in the two barometers happens not to exceed $\frac{1}{10}$ or even $\frac{2}{10}$ of an inch (and this will frequently be the cafe in levelling flat countries, or meafuring fmall heights) in fuch circumftances the moft convenient way of reducing the obfervations will be by means of the column of differences only; thofe numbers expreffing the length of a column of the atmofphere which correfponds to $\frac{1}{10}$ of an inch of quickfilver, at any affigned height of the barometer.

EXAMPLE IV.

Suppofe the following obfervations had been made at the top and bottom of any eminence; *viz.* at the top, barometer 29.985 inches, attached thermometer 70°5, detached thermometer 76°; and below, barometer at 30.082, attached thermometer 71°, and the detached one 68°; what was the height of the eminence?

Obſervation below.

Barometer.	Therm. attached.	Therm. detached.
In.	°	°
30.0820	71.0	68.0
	70.5	

Difference of the two attached therm. 0.5

Obſervation at the top.

	Barometer.	Therm. attached.	Therm. detached.
	In.	°	°
	29.9850	70.5	76.0
Correct for 0°.5 of heat,	+.0015		68.0

Take — —	29.9865	2)144.0(72.0 mean heat.
From — —	30.0820	31.2 ſtandard temp.
Remains the difference or fall of quickſilver in the barometer, —	0.0955	+40.8 difference.

The difference for $\frac{1}{10}$ at 30 inches = 86.7 feet.

	Feet.
Therefore, for 0900 — —	78.0
0050 — —	4.3
0005 — —	0.4

Correction for 41°.	Feet.	Ft.
41° on	80	= 8.0
	2.7	= .3
41° on	82.7	= 8.3

Therefore, 0955 inch of quickſilver, —	82.7	the approximate height.
Correction for 41° on 82.7 feet,	+8.3	
Gives — — —	91.0	= the true height.

Now this was the height of the Tarpeian rock, or the weſt-end of the Capitol-hill in Rome, above the convent of St. Clare, in the *Strada dei ſpecchi*.

The preceding rules for determining heights above the ſurface of the earth will, I preſume, anſwer equally well for meaſuring depths below it.

TABLE

TABLE I. For the expansion of quicksilver by heat, see p. 574.

Degr. of the Therm.	Height of the barometer in inches.												
	20	21	22	23	24	25	26	27	28	29	30	31	32
1	2 0	2.1	2.2	2.3	2.4	2.5	2 6	2.7	2.8	2 9	3.0	3.1	3.2
2	4.1	4.3	4.5	4.7	4 9	5.1	5.3	5.5	5.7	5.9	6.1	6.3	6.5
3	6.1	6.4	6.7	7.0	7.3	7.6	7.9	8 2	8.5	8.8	9.1	9.4	9 7
4	8.1	8.5	8.9	9.3	9.7	10.1	10.5	11.0	11.4	11.8	12.2	12.6	13.0
5	10.1	10.6	11.1	11.6	12.1	12.7	13.2	13.7	14.2	14.7	15.2	15.7	16.2
6	12.2	12.8	13.4	14 0	14.6	15.2	15.8	16.4	17.0	17.6	18.2	18.8	19.5
7	14.2	14.9	15.6	16.3	17.0	17.7	18.4	19.2	19.8	20.6	21.3	22.0	22.7
8	16.2	17.0	17.8	18.6	19.4	20.2	21.0	21.9	22.7	23.5	24.3	25.2	25.9
9	18.2	19.2	20 1	21.0	21.9	22.8	23.7	24.6	25.6	26.5	27.4	28.3	29.2
10	20.3	21.3	22.3	23.3	24.3	25.3	26.3	27.4	28.4	29.4	30.4	31.4	32.4
11	22.3	23.4	24.5	25.6	26.7	27.8	28.9	30.1	31.2	32.3	33.4	34.5	35.6
12	24.3	25.6	26.8	28.0	29.2	30.4	31.6	32.9	34.1	35.3	36.5	37.6	38.9
13	26.3	27.7	29.0	30.3	31.6	32.9	34.2	35.6	36.9	38.2	39.5	40.8	42.1
14	28.4	29.8	31.2	32.6	34.0	35 4	36.8	38.4	39.8	41.2	42.6	43.9	45.4
15	30.4	31.9	33.4	34.9	36.4	37.9	39.4	41.1	42.6	44.1	45.6	47.1	48 6
16	32.4	34.1	35.6	37.2	38.8	40.5	42.0	43.8	45 4	47.0	48.6	50.3	51.8
17	34.5	36.2	37.9	39.6	41.3	43.0	44.7	46.6	48.3	50.0	51.7	53.4	55.1
18	36.5	38.3	40.1	41.9	43.7	45.5	47.3	49.3	51.1	52.9	54.7	56.5	58.3
19	38.5	40.5	42.3	44.2	46.1	48.1	49.9	52.1	54.0	55.9	57.8	59.7	61.6
20	40.6	42.6	44.6	46.6	48.6	50.6	52.6	54.8	56.8	58 8	60.8	62.8	64.9
21	42.6	44.7	46.8	48.9	51 0	53.2	55.2	57 5	59.6	61.7	63 8	65.9	68.1
22	44.6	46.9	49.1	51.3	53.5	55.7	57.9	60.3	62.5	64.7	66.9	69 0	71.4
23	46.6	49 0	51.3	53.6	55.9	58.2	60.5	63.0	65.3	67.6	69.9	72.2	74.6
24	48.6	51.1	53.5	55.9	58.3	60.8	63.1	65.8	68.2	70.6	73 0	75 4	77.8
25	50.7	53.2	55.8	58.2	60.7	63.2	65.7	68.5	71.0	73.5	76 0	78.5	81.1
26	52.7	55.4	58.0	60.5	63.1	65.8	68.3	71 2	73.8	76.4	79 0	81.6	84.3
27	54.7	57.5	60.3	62.9	65.6	68.3	71.0	74.0	76.7	79.4	82.1	84.8	87.5
28	56.8	59.6	62.5	65.2	68.0	70.8	73.6	76.7	79.5	82.3	85.1	87.9	90.7
29	58.8	61.8	64.7	67.5	70.4	73.3	76.2	79.5	82.4	85.3	88.2	91.1	94.1
30	60.8	63.9	66.9	69.9	72.8	75.9	78.9	82.2	85.2	88.2	91 2	94.1	97.3
31	62.8	66.0	69.1	72.2	75.2	78 4	81.5	84.9	88 0	91.1	94.2	97.4	100.5
32	64.8	68 2	71.4	74.6	77.7	81.0	84.2	87.7	90.9	94.1	97.3	100.5	103.8
33	66.9	70.3	73.6	76.9	80.1	83.5	86.8	90.4	93.7	97.0	100.3	103.0	107.0
34	68.9	72.4	75.8	79.2	82.5	86.1	89.4	93.2	96.6	100.0	103.4	106.7	110.3
35	70.9	74.5	78.0	81.5	84.0	88.6	92.0	95.9	99.4	102.9	106.4	109.9	113 5
36	73.0	76.7	80.2	83.8	86.4	91.1	94.6	98.6	102.2	105.8	109.4	113.1	116.8
37	75.0	78.8	82.5	86.2	88.9	93.6	97.3	101.4	105.1	108.8	112.5	116.2	120.0
38	77.0	80.9	84.7	88.5	91.3	96.2	99.9	104.1	107.9	111.7	115.5	119.3	123.2
39	79.0	83.1	86.9	90.8	93.7	98.7	102.5	106.9	110.8	114.7	118.6	122.5	126.5
40	81.1	85.2	89.2	93 2	97.2	101.2	105.2	109.6	113.6	117.6	121.6	125 6	129.7

TABLE II[(f)]. Giving the approximate height in English feet, adapted to the temperature 31°24 of FAHRENHEIT'S thermometer.

Height of the Barom.	Height.	Diff.	Height of the Barom.	Height.	Diff.	Height of the Barom.	Height.	Diff.
Inch.	Feet.		Inch.	Feet.		Inch.	Feet.	
1.—	90309.0	18062	16.10	19570.4	173.1	16.60	17102.5	157.5
2.—	72247.2	10565	20	19398 4	172.0	70	16946.0	156.5
3.—	61681.8	7496	30	19227.5	170.9	80	16790.4	155.6
4.—	54185 4	5814	40	19057.7	169.8	90	16635.8	154.6
5.—	48370.8	4761	50	18889.1	168.6	17 00	16482.1	153 7
6.—	43619.9	4017	60	18721.5	167 6	10	16329.2	152.9
7.—	39603.1	3480	70	18555.0	166.5	20	16177.3	151.9
8.—	36123.6	3069	80	18389.6	165.4	30	16026.2	151.1
9.—	33054.4	2745	90	18225.2	164.4	40	15876.0	150.2
10.—	30309.0	2484	16.00	18061.8	163 4	50	15726.7	149.3
11.—	27825.4	2267	10	17899.4	162.4	60	15578.2	148.5
12.—	25558.1	2086	20	17738.1	161.3	70	15430.6	147.6
13.—	23472.4	1931	30	17577.7	160.4	80	15283.8	146.8
14.—	21541.3	1798	40	17418.4	159.3	90	15137.8	146.0
15.00	19743.5		50	17260.0	158.4	18.00	14992 6	145.2

(*f*) This table bears some analogy to the tables of logistical logarithms, being nothing more than the differences of the logarithms of the height of the barometer from the logarithm of 32 inches multiplied by six. I have chosen the logarithm of 32 for my term of comparison, that being the greatest probable height that the barometer will ever be seen at, even at the bottom of the deepest mines. Had I taken the mean height of the quicksilver at the level of the sea, it is true the numbers in the table would have more truly represented the heights in the atmosphere, corresponding to the given height of the quicksilver; but then, in computing small depths or heights from the surface of the sea, we should have been obliged sometimes to have changed the signs in the operation, which appeared to me less convenient. The mean height of the barometer at the level of the sea, from 132 observations in Italy and in England, is 30.04 inches, the heat of the barometer being 55°, and the air 62°; so that the term of comparison in this table, *viz.* 32 inches, corresponds to an imaginary point within the earth at 1647 feet below the surface of the sea.

TABLE

TABLE II. continued.

Height of the Barom.	Height.	Diff.	Height of the Barom.	Height.	Diff.	Height of the Barom.	Height.	Diff.
Inch.	Feet.		Inch.	Feet.		Inch.	Feet.	
18.10	14848.3	144.3	22.00	9763.6	118.8	25.90	5511.0	100.8
20	14704.7	143.6	10	9645.5	118.1	26.00	5410.4	100.6
30	14561.9	142.8	20	9527.8	117.7	10	5310.6	99.8
40	14419.9	142.0	30	9410.7	117.1	20	5210.9	99.7
50	14278.7	141.2	40	9294.1	116.6	30	5111.6	99.3
60	14138.2	140.5	50	9178.1	116.0	40	5012.8	98.8
70	13998.5	139.7	60	9062.5	115.6	50	4914.2	98.6
80	13859.5	139.0	70	8947.4	115.1	60	4816.4	98.1
90	13721.3	138.2	80	8832.9	114.5	70	4718.3	97.8
19.00	13583.8	137.5	90	8718.9	114.0	80	4620.9	97.4
10	13447.0	136.8	23.00	8605.3	113.6	90	4523.9	97.0
20	13310.9	136.1	10	8492.3	113.0	27.00	4427.2	96.7
30	13175.6	135.3	20	8379.7	112.6	10	4330.8	96.4
40	13041.1	134.5	30	8267.6	112.1	20	4234.9	95.9
50	12906.9	134.2	40	8156.0	111.6	30	4139.2	95.7
60	12773.6	133.3	50	8044.9	111.1	40	4044.0	95.2
70	12641.0	132.6	60	7934.3	110.6	50	3949.0	95.0
80	12509.1	131.9	70	7824.1	110.2	60	3854.5	94.5
90	12337.0	131.3	80	7714.4	109.7	70	3760.2	94.3
20.00	12247.2	130.6	90	7605.1	109.3	80	3666.3	93.9
10	12117.2	130.0	24.00	7496.3	108.8	90	3572.7	93.6
20	11987.9	129.3	10	7388.0	108.3	28.00	3479.5	93.2
30	11859.2	128.7	20	7280.1	107.9	10	3386.6	92.9
40	11731.2	128.0	30	7172.6	107.5	20	3294.0	92.6
50	11603.8	127.4	40	7065.6	107.0	30	3201.8	92.2
60	11477.0	126.8	50	6959.0	106.6	40	3109.9	91.9
70	11350.0	126.2	60	6852.9	106.1	50	3018.3	91.6
80	11225.2	125.6	70	6747.2	105.7	60	2927.0	91.3
90	11100.2	125.0	80	6641.9	105.3	70	2836.1	90.9
21.00	10975.8	124.4	90	6537.0	104.9	80	2745.4	90.7
10	10852.1	123.7	25.00	6432.6	104.4	90	2655.1	90.3
20	10728.8	123.3	10	6328.6	104.0	29.00	2565.1	90.0
30	10606.2	122.6	20	6225.0	103.6	10	2475.4	89.7
40	10484.2	122.0	30	6121.8	103.2	20	2386.0	89.4
50	10362.7	121.5	40	6019.0	102.8	30	2296.9	89.1
60	10241.8	120.9	50	5916.6	102.4	40	2208.2	88.7
70	10121.4	120.4	60	5814.6	102.0	50	2119.7	88.5
80	10001.6	119.8	70	5713.0	101.6	60	2031.5	88.2
90	9882.4	119.2	80	5611.8	101.2	70	1943.6	87.9

TABLE II. continued.

Height of the Barom.	Height.	Diff.	Height of the Barom.	Height.	Diff.	Height of the Barom.	Height.	Diff.
Inch.	Feet.		Inch.	Feet.		Inch.	Feet.	
29.80	1856.0	87.6	30.60	1165.7	85.3	31.40	493.2	83.1
90	1768.7	87.3	70	1080.7	85.0	50	410.4	82.8
30.00	1681.7	87.0	80	996.0	84.7	60	327.8	82.6
10	1595.0	86.7	90	911.5	84.5	70	245.4	82.4
20	1508.6	86.4	31.00	827.3	84.2	80	163.4	82.0
30	1422.4	86.2	10	743.4	83.9	90	81.6	81.0
40	1236.6	85.8	20	659.7	83.7	32.00	00.0	81.6
50	1251.0	85.6	30	576.3	83.4			

TABLE

TABLE III. Of proportional parts.

Diff.	1	2	3	4	5	6	7	8	9	Diff.	1	2	3	4	5	6	7	8	9
81	8	16	24	32	40	49	57	65	73	106	11	21	32	42	53	64	74	85	95
82	8	16	25	33	41	49	57	66	74	107	11	21	32	43	53	64	75	86	96
83	8	17	25	33	41	50	58	66	75	108	11	22	3[illegible]	43	54	65	76	86	97
84	8	17	25	34	42	50	59	67	76	109	11	22	33	44	54	65	76	87	98
85	8	17	25	34	42	51	59	68	76	110	11	22	33	44	55	66	77	88	99
86	9	17	26	34	43	52	60	69	77	111	11	22	33	44	55	67	78	89	100
87	9	17	26	35	43	52	61	70	78	112	11	22	34	45	56	67	78	90	101
88	9	18	26	35	44	53	62	70	79	113	11	23	34	45	5[illegible]	68	79	90	102
89	9	18	27	36	44	53	62	71	80	114	11	23	34	46	5[illegible]	68	80	91	103
90	9	18	27	36	45	54	63	72	81	115	11	23	34	46	57	69	80	92	103
91	9	18	27	36	45	55	64	73	82	116	12	23	35	46	58	70	81	93	104
92	9	18	28	37	46	55	64	74	83	117	12	23	35	47	58	70	82	94	105
93	9	19	28	37	46	56	65	74	84	118	12	24	35	47	59	71	83	94	106
94	9	19	28	38	47	56	66	75	85	119	12	24	36	4[illegible]	59	71	83	95	107
95	9	19	28	38	47	57	66	76	85	120	12	24	36	48	60	72	84	96	108
96	10	19	29	38	48	58	67	77	86	121	12	24	36	48	60	73	85	97	109
97	10	19	29	39	48	58	68	78	87	122	12	24	37	49	61	73	85	98	110
98	10	20	29	39	49	59	69	78	88	123	12	25	37	49	61	74	86	98	111
99	10	20	30	40	49	59	69	79	89	124	12	25	37	50	62	74	87	99	112
100	10	20	30	40	50	60	70	80	90	125	12	25	37	50	62	75	87	100	112
101	10	20	30	40	50	61	71	81	91	126	13	25	38	50	63	76	88	101	113
102	10	20	31	41	51	61	71	82	92	127	13	25	38	51	63	76	89	102	114
103	10	21	31	41	51	62	72	82	93	128	13	26	3[illegible]	51	64	77	90	102	115
104	10	21	31	42	52	62	73	83	94	129	13	26	39	52	64	77	90	103	116
105	10	21	31	42	52	63	73	84	94	130	13	26	39	52	65	78	91	104	117

TABLE IV. For the expanſion of the air, or correc tion of the uppermoſt height, ſee p. 576.

Deg.	Approximate height in feet.								
0	1000.	2000.	3000.	4000.	5000.	6000.	7000.	8000.	9000.
1	2.4	4.9	7.3	9.7	12 1	14 6	17.0	19.4	21.9
2	4.9	9.7	14.6	19 4	24.3	29 2	34 0	38.9	43 7
3	7.3	14.6	21.9	29.2	36 4	43.7	51.0	58.3	65 6
4	9.7	19.4	29.2	38.9	48.6	58 3	68.0	77.8	87.5
5	12.1	24.3	36.4	48.6	60.7	72.9	85.0	97.2	109.3
6	14.6	29.2	43.7	58.3	72.8	87.5	102.0	116.6	131.2
7	17.0	34.0	51.0	68.0	85.0	102.1	119.0	136.1	153.0
8	19.4	38.9	58.3	77.8	97.1	116.6	136.0	155.5	174.9
9	21.9	43.7	65.6	87.5	109.3	131.2	153.0	175.0	196.8
10	24.3	48.6	72.9	97.2	121.5	145 8	170.1	194 4	218.7
11	26.7	53.5	80.2	106.9	133 6	160.4	187.1	213 8	240.6
12	29.2	58.3	87.5	116 6	145.8	175.0	204.1	233.3	262 4
13	31.6	63 2	94.8	126.4	157.9	189.5	221.1	252.7	284.3
14	34 0	68.0	102.1	136.1	170.1	204.1	238.1	272.2	306.2
15	36.4	72.9	109.3	145.8	182.2	218.7	255.1	291.6	328.0
16	38.8	77.8	116.6	155.5	194.3	233.3	272.1	311,0	349.9
17	41.3	82.6	123.9	165.2	206.5	247.9	289.1	330.5	371.7
18	43.7	87.5	131.2	175.0	218.6	262.4	306.1	349 9	393.6
19	46.1	92 3	138.5	184.7	230.8	277.0	323.1	369.4	415.5
20	48.6	97.2	145.8	194 4	243 0	291.6	340.2	388 8	437.4
21	51.0	102.1	153.1	204.1	255.1	306.2	357.2	408.2	459.3
22	53.5	106 9	160.4	213.8	267.3	320.8	374 2	427.7	481.1
23	55.9	111 8	167.7	223.6	279.4	335.3	391.2	447 1	503.0
24	58.3	116.6	175.0	233.3	291 6	349 9	408.2	466.6	524.9
25	60.7	121.5	182.2	243 0	303.7	364 5	425 2	486 0	546.7

TABLE IV. continued.

Deg.	Approximate height in feet.								
°	1000	2000.	3000.	4000	5000.	6000.	7000.	8000	9000.
26	63.1	126.4	189.5	252.7	315.8	379.1	442.2	505.4	568.6
27	65.6	131.2	196.8	262.4	328.0	393.7	459.2	524.9	590.4
28	68.0	136.1	204.1	272.2	340.1	408.2	476.2	544.3	612.3
29	70.4	140.9	211.4	281.9	352.3	422.8	493.2	563.8	634.2
30	72.9	145.8	218.7	291.6	364.5	437.4	510.3	583.2	656.1
31	75.3	150.7	226.0	301.3	376.6	452.0	527.3	602.6	678.0
32	77.8	155.5	233.3	311.0	388.8	466.6	544.3	622.1	699.8
33	80.2	160.4	240.6	320.8	400.9	480.1	561.3	641.5	721.7
34	82.6	165.2	247.9	330.5	413.1	495.7	578.3	661.0	743.6
35	85.0	170.1	255.1	340.2	425.2	510.2	595.3	680.4	765.4
36	87.4	175.0	262.4	349.9	437.3	524.8	612.3	699.8	787.3
37	89.9	179.8	269.7	359.6	449.5	539.4	629.3	719.3	809.1
38	92.3	184.7	277.0	369.4	461.6	553.9	646.3	738.7	831.0
39	94.7	189.5	284.3	379.1	473.8	568.5	663.3	758.2	852.9
40	97.2	194.4	291.6	388.8	486.0	583.2	680.4	777.6	874.8
41	99.6	199.3	298.9	398.5	498.1	597.8	697.4	797.0	896.7
42	102.1	204.1	306.2	408.2	510.3	612.4	714.4	816.5	918.5
43	104.5	209.0	313.5	418.0	522.4	626.9	731.4	835.9	940.4
44	106.9	213.8	320.8	427.7	534.6	641.5	748.4	855.4	962.3
45	109.3	218.7	328.0	437.4	546.7	656.1	765.4	874.8	984.1
46	111.7	223.6	335.3	447.1	558.8	670.7	782.4	894.2	1006.0
47	114.2	228.4	342.6	456.8	571.0	685.3	799.4	913.7	1027.8
48	116.6	233.3	349.9	466.6	583.1	699.8	816.4	933.1	1049.7
49	119.0	238.1	357.2	476.3	595.3	714.4	833.4	952.6	1071.6
50	121.5	243.0	364.5	486.0	607.5	729.0	850.5	972.0	1093.5

Table of heights taken by the barometer, &c.

	+ or — the Lake of Geneva. Feet.	Above the Mediterranean. Feet.
The Lake of Geneva, from 18 obfervations, —	0	1230 *(g)*
Greateft depth of the Lake, — —	— 393	
Clufe, at the Croix Blanche, firft-floor, *(h)* 2,	+ 351	1581
Chamouny, ground-floor of the inn near the foot of Mont Blanc, 4 — — —	+ 2137	3367
The Montanvert, at the Chateâu, 1 —	+ 5001	6231
The fource of the river Arvéron, at the bottom of the Vallée de Glace, 1 — — —	+ 2426	3656
Salenche, at the inn, fecond-floor, 1 —	+ 664	1941
La Bonne-Ville, a la Ville de Geneve, fecond floor, 1	+ 245	1475
Chatlaino, country houfe near Geneva, g ound-floor, G	+ 178	
The ball on the higheft, or fouth-weft, tower of St. Peter's church in Geneva, G — —	+ 249	
St. Joire, in a field at the foot of the Mole, G —	+ 671	1901
Summit of the Mole, — — — —	+ 4883	6113
Pitton, higheft point of Mont Saleve, G —	3284	4514
The Dole, higheft fummit of Mont Jura, G —	+ 4293	5523
The Buet, G — — — — —	+ 8894	10124
Aiguille d'Argentiére, G — — —	+ 12172	13402
Mont Blanc, G — — —	+ 14432	15662
Frangy, at the inn, firft-floor, below the Lake,	— 166	
Aix, a la Ville de Geneve, firft-floor, below the Lake,	— 378	
Chambery, au St. Jean Baptifte, firft-floor, below the Lake,	— 352	
Aiguebelle, at the inn, firft-floor, below the Lake,	— 190	
La Chambre, at the inn, firft-floor, above the Lake,	+ 337	
St. Michael, at the inn, firft-floor, —	+ 1113	2343
Modane, at the inn, firft floor, — —	+ 2220	3450

(g) More correctly 1228 feet, but I have taken it at 1230 in round numbers.

(h) The figures at the end of fome of the names fhew the number of obfervations that were made; and the letter G indicates fuch obfervations to have been geometrical.

Table

Table of heights, &c. continued.

		+ or — the Lake of Geneva. Feet.	Above the Mediterranean. Feet.
Lannebourg, the foot of Mont Cenis, at the inn, first-floor,	+	3178	4408
Mont Cenis, at the Post	+	5031	6261
———— at the Grande Croix,	+	4793	6023
Novalese, the foot of Mont Cenis on the side of Italy, at the inn, first-floor,	+	1511	2741
Boucholin, on the first-floor,	+	213	
St. Ambroise, on the first-floor, below the Lake,	—	40	
Turin, a l'Hôtel d'Angleterre, second-floor, 4	—	289	941
Felissano, near Alessandria, first-floor, 1	—	671	
Piacenza, St. Marco, first-floor, 1	—	967	263
Parma, au Paon, first-floor, 3	—	923	307
Bologna, au Pelerin, first-floor, 3	—	831	399
Loiano, a little village on the Appenines, between Bologna and Florence,			2591
The mountain Raticosa, the highest point of the Appenines the road passes over, $1\frac{1}{4}$ miles beyond Filecaije in going to Covigliaje,	+	1671	2901
Florence, nel Corso dei Tintori, 50 feet above the Arno, which was 18 feet below the wall of the quay, 3	—	990	+240
Pisa, aux Trois Demoiselles, second-floor, 4	—	1228	$+54\frac{1}{2}$
Leghorn, chez Muston, second-floor, 2	—	1244	+38
Siena, aux Trois Rois, second-floor, 2	—	164	1066
Redicoffani, at the Post, first-floor, above the Lake,	+	1240	2470
———— the top of the tower of the old fortification on the summit of the rock,		1830	3060
Viterbo, aux Trois Rois, first-floor, on the Ciminus of the Ancients,	+	29	1259
Rome, nel Corso, 61 feet above the Tyber, 7	—	1084	94

(*i*) The rocks on each side the plain, where the post-house stands, are at least 3000 feet higher than this situation; and it is from the snow on the tops, and through the crevices, that the lake on this plain is formed, which gives rise to the Dora, and may be called one of the sources of the Po.

Table of heights, &c. continued.

	Above the River Tyber. Feet.	Above the Mediterranean. Feet.
The Level of the river Tyber, — —		33
The top of the Janiculum, near the Villa Spada, —	260	
Aventine Hill, near the Priory of Malta, —	117	
In the Forum, near the arch of SEVERUS, where the ground is raiſed $23\frac{1}{2}$ feet, — —	34	
Palatine Hill, on the floor of the Imperial palace —	133	
Celian Hill, near the CLAUDIAN aqueduct, —	125	
Bottom of the canal of the CLAUDIAN aqueduct, —	175	
Eſquiline Hill, on the floor of St. M. Major's church,	154	
Capitol Hill, on the Weſt-end of the Tarpeian rock,	118	
In the Strada dei Specchi, in the convent of St. Clare,	27	
On the union of the Viminal and Quirinal Hills, in the Carthuſian's church, DIOCLES. Baths, —	141	
Pincian Hill, in the garden of the Villa Medici, —	165	
Top of the croſs of St. Peter's church, —	502	
The baſe of the obeliſk, in the center of the Periſtyle,	31	
The ſummit of the mountain Soracte, lying about $20\frac{1}{2}$ geog. miles N. of Rome, G — —		2271
The ſummit of Monte Velino, one of the Appenines, covered with ſnow in June, about 46 geog. miles N.W. of Rome, and which is probably the higheſt of the Appenines, G — — —		8397
	+ or — the Lake of Geneve.	
Naples, Caſa Iſolata on the Chiaia, $27\frac{1}{2}$ feet above the ſea, 5 — — —	— 1197	
Mount Veſuvius, mouth of the Crater from whence the fire iſſued in 1776, — —		3938 (*k*)

Table

(*k*) Sir WILLIAM HAMILTON informed me, that the height of Veſuvius, as taken by Mr. DE SAUSSURE of Geneva in 1772, with only a barometer of Mr. DE LUC's conſtruction, and according to his rules, was $3659\frac{1}{2}$ French feet = 3900 Engliſh, which agrees pretty well with mine. But the Padre DELLA TORRE pretends to have found the height of Veſuvius in 1752 (ſee p. 44. of his

Table of heignts, &c. continued.

	+ or — the Lake of Geneva.	Above the Mediterranean.
Mount Vesuvius, at the base of the cone,	—	2021
Top of the mountain Somma,	—	3738
The summit of Mount Ætna,	—	10954 (l)

The following heights are determined from corresponding observations by Mr. MESSIER at Paris, whose barometer is supposed 108 feet above the sea.

Barberino di Valdensa, between Boggebonri and Tavernelle,	974
Modena, a l'Albergo nuovo, — —	214
Montmelian, at 20 feet above the river, — —	811
Monte Viso, by an observation from Jurin, by means accurate, G — — —	9997
Monte Rosa, as measured geometrically by the Father BECCARIA, being the second mountain of all the Alps, — — — —	15084
Pont Beauvoisin, — — — —	705
La tour du Pin, 4 — — —	938
Verpilliére, — — — —	566

his History of this Mountain) = 1677 French feet only, the difference of his barometer at the top and at the level of the sea being no more than $23\frac{1}{4}$ French lines = 2.065 English inches, which was certainly a mistake of little less than 2000 feet in the result. The Abbé NOLLET in 1749 found the fall of the quicksilver 40 lines = 3.55 inches English; and, if this observation is to be depended upon, the summit of this volcano has risen within these 27 years more than 330 feet perpendicular.

(l) I have ventured to compute the height of this celebrated mountain from my own tables, though from an observation of Mr. DE SAUSSURE's in 1773, which that gentleman obligingly communicated to me. It will serve to shew, that this volcano is by no means the highest mountain of the old world; and that Vesuvius, placed upon Mount Ætna, would not be equal to the height of Mont Blanc, which latter I take to be the most elevated point in Europe, Asia, or Africa.

The circumference of the visible horizon on the top of Mount Ætna, allowance being made for refraction, which I estimate at 6′, is 1093 English miles.

Table of heights, &c. continued.

	+ or — the Lake of Geneva.	Above the Mediterranean.
Lyons, at the Hôtel Blanc, 50 feet above the Saône,		449
St. Jean le vieux, — — —		695
Cerdon, near the poſt-houſe at the foot of the rocks,		854
Nantua, 10 feet above the Lake, — —		1423
Chatillon, at the Logis Neuf, — —		1629
Colonges, — — — —		1626
St. Genis, apparently on a level with the foot of Mont Jura,		1501
Geneva, at 100 feet above the Lake, 5 —		1268 (m)
Mâcon, at the Parc, 24 feet above the Saône —		514
Dijon, à la Cloche, the firſt-floor, — —		710
Mountain of Maraiſelois (n), $4\frac{3}{4}$ miles beyond Viteaux towards Dijon, — —		1677
Lucy-le-bois, — — — —		645
Auxerre, 50 feet above the river, — —		283
Sens, at the Poſt, — — —		163
Fontainbleau, at the Grand Cerf, ſecond-floor, —		242

(m) From this compariſon with Mr. MESSIER's obſervations at Paris, which makes the Lake of Geneva only 1168 feet above the level of the ſea (whereas from 18 obſervations in Italy, near the ſhore of the Mediterranean, it appears to be 1228, *viz.* +60 feet different) I am inclined to believe, that Mr. MESSIER's place of obſervation is about 50 feet higher than I have ſuppoſed it, *viz.* 160 feet above the ſea inſtead of 108, as deduced from three obſervations only at Boulogne, Calais, and at Dover. If this be allowed, the ſame number of feet muſt be added alſo to all the other heights that are determined by compariſon with Mr. MESSIER's obſervations. I am, however, by no means ſure of this, but leave it to future obſervers.

(n) On one ſide of this mountain is a little ſtream called Amançon, that joins the Yonne and the Seine, and thus goes to the Atlantic; while on the other ſide is found the Ouche, which, uniting with the Saône and the Rhone, runs to the Mediterranean: this part of Burgundy then ſeems to be one of the higheſt in France.

Table of heights, &c. continued.

	+ or — the Seine at Paris.	Above the Mediterranean.
Paris, mean height of the Seine, that is, *quand les eaux ſe trouvent à* 13 *pieds* 9 *pouces ſur l'echelle du Pont Royal ſelon M.* DE LA LANDE, —		$36\frac{1}{2}$
Place of my own obſervations in the Ruë Jacob, ſecond-floor, — — —	+ 57	
Mr. MESSIER's obſervatory, at the Hôtel de Clugny, firſt floor, — — —	72	
Mr. DE LA LANDE's ditto, at the College Royal, firſt-floor,	101	
Place of Monſ. le Pere COTTES's obſervations at Montmorency, 10 miles North of Paris, —	333	
Stone-gallery of the Church on Mont Valerien,	473	
Depth of the cave of the Royal Obſervatory at Paris below the pavement, — —	$98\frac{1}{2}$	
The ſame, according to Mr. DE LA LANDE, by actual meaſurement, — — —	98	
Height of the north tower of the church of Notre Dame above the floor, — —	$220\frac{1}{2}$	
——— by actual meaſurement, — —	$218\frac{1}{2}$	
Chantilly, — — — —		119
Clermont, — — — —		329
Amiens, Ruë de Noyon, firſt-floor, — —		147
Abbeville, firſt-floor, — — —		79
	Below the mean height of the Seine.	
Boulogne, mean level of the ſea, from one obſerv. only,	—33.9	
Calais, ditto, from one obſervation, — —	—38.8	
Dover, ditto, from three obſervations made two years preceding thoſe at Calais and Boulogne, —	—36.6	
Mean height of the river (o) Thames at London above the mean height of the river Seine from five direct compariſons with Mr. MESSIER, —	+ 6.8	
And conſequently the Thames at London above the ſea,		43
Warwick, mean level of the river Avon, —		155
Shuckburgh-houſe, in Warwickſhire, — —		560

(*o*) By the mean height of the river Thames is underſtood when the water is $15\frac{1}{2}$ feet below the pavement in the left-hand arcade at Buckingham-ſtairs.

XXX. *An Account of the* Bramin's *Obſervatory at* Benares. *By Sir* Robert Barker, *Knt. F. R. S.*; *in a Letter to Sir* John Pringle, *Bart. P. R. S.*

SIR,

Read May 29, 1777.

BENARES in the Eaſt Indies, one of the principal ſeminaries of the Bramins or prieſts of the original Gentoos of Hindoſtan, continues ſtill to be the place of reſort of that ſect of people; and there are many publick charities, hoſpitals, and pagodas, where ſome thouſands of them now reſide. Having frequently heard that the ancient Bramins had a knowledge of aſtronomy, and being confirmed in this by their information of an approaching eclipſe both of the Sun and Moon, I made inquiry, when at that place in the year 1772, among the principal Bramins, to endeavour to get ſome information relative to the manner in which they were acquainted of an approaching eclipſe. The moſt intelligent that I could meet with, however, gave me but little ſatisfaction. I was told, that theſe matters were confined to a few, who were in poſſeſſion of certain books and records; ſome containing the myſteries

teries of their religion, and others the tables of aſtronomical obſervations, written in the Skanſkirrit language, which few underſtood but themſelves: that they would take me to a place which had been conſtructed for the purpoſe of making ſuch obſervations as I was inquiring after, and from whence they ſuppoſed the learned Bramins made theirs. I was then conducted to an ancient building of ſtone, the lower part of which, in its preſent ſituation, was converted into a ſtable for horſes, and a receptacle for lumber; but, by the number of court-yards and apartments, it appeared that it muſt once have been an edifice for the uſe of ſome public body of people. We entered this building, and went up a ſtaircaſe to the top of a part of it, near to the river Ganges, that led to a large terrace, where, to my ſurprize and ſatisfaction, I ſaw a number of inſtruments yet remaining, in the greateſt preſervation, ſtupendouſly large, immoveable from the ſpot, and built of ſtone, ſome of them being upwards of twenty feet in height; and, although they are ſaid to have been erected two hundred years ago, the graduations and diviſions on the ſeveral arcs appeared as well cut, and as accurately divided, as if they had been the performance of a modern artiſt. The execution in the conſtruction of theſe inſtruments exhibited a mathematical exactneſs in the fixing, bearing, and

fitting

fitting of the ſeveral parts, in the neceſſary and ſufficient ſupports to the very large ſtones that compoſed them, and in the joining and faſtening each into the other by means of lead and iron.

The ſituation of the two large quadrants of the inſtrument marked A in the firſt plate, whoſe radius is nine feet two inches, by their being at right angles with a gnomon at twenty-five degrees elevation, are thrown into ſuch an oblique ſituation as to render them the moſt difficult, not only to conſtruct of ſuch a magnitude, but to ſecure in their poſition for ſo long a period, and affords a ſtriking inſtance of the ability of the architect in their conſtruction; for, by the ſhadow of the gnomon thrown on the quadrants, they do not appear to have altered in the leaſt from their original poſition; and ſo true is the line of the gnomon, that, by applying the eye to a ſmall iron ring of an inch diameter at one end, the ſight is carried through three others of the ſame dimenſion to the extremity at the other end, diſtant thirty-eight feet eight inches, without obſtruction; ſuch is the firmneſs and art with which this inſtrument has been executed. This performance is the more wonderful and extraordinary when compared with the works of the artificers of Hindoſtan at this day, who are not under the immediate

direction

direction of an European mechanic; but arts appear to have declined equally with ſcience in the Eaſt.

Lieutenant-colonel ARCHIBALD CAMPBELL, at that time chief engineer in the Eaſt India Company's ſervice at Bengal, a gentleman whoſe abilities do honour to his profeſſion, made a perſpective drawing of the whole of the apparatus that could be brought within his eye at one view; but I lament he could not repreſent ſome very large quadrants, whoſe radii were about twenty feet, they being on the ſide from whence he took his drawing. Their deſcription however is, that they are exact quarters of circles of different radii, the largeſt of which I judged to be twenty feet, conſtructed very exactly on the ſides of ſtone walls built perpendicular, and ſituated, I ſuppoſe, in the meridian of the place: a braſs pin is fixed at the center or angle of the quadrant, from whence, the Bramin informed me, they ſtretched a wire to the circumference when an obſervation was to be made; from which it occurred to me, the obſerver muſt have moved his eye up or down the circumference, by means of a ladder or ſome ſuch contrivance, to raiſe and lower himſelf, until he had diſcovered the altitude of any of the heavenly bodies in their paſſage over the meridian, ſo expreſſed on the arcs of theſe quadrants: theſe arcs were very exactly divided into

 nine

nine large fections; each of which again into ten, making ninety leffer divifions or degrees; and thofe alfo into twenty, expreffing three minutes each, of about two-tenths of an inch afunder; fo that it is probable, they had fome method of dividing even thefe into more minute divifions at the time of obfervation.

My time would only permit me to take down the particular dimenfions of the moft capital inftrument, or the greater equinoctial Sun-dial, reprefented by figure A, plate 12. which appears to be an inftrument to exprefs folar time by the fhadow of a gnomon upon two quadrants, one fituated to the eaft, and the other to the weft of it; and indeed the chief part of their inftruments at this place appear to be conftructed for the fame purpofe, except the quadrants, and a brafs inftrument that will be defcribed hereafter.

Figure B is another inftrument for the purpofe of determining the exact hour of the day by the fhadow of a gnomon, which ftands perpendicular to and in the center of a flat circular ftone, fupported in an oblique fituation by means of four upright ftones and a crofs-piece; fo that the fhadow of the gnomon, which is a perpendicular iron rod, is thrown upon the divifions of the circle defcribed on the face of the flat, circular ftone.

5 Figure

Figure C is a brafs circle, about two feet diameter, moving vertically upon two pivots between two ftone pillars, having an index or hand turning round horizontally on the center of this circle, which is divided into 360 parts; but there are no counter divifions on the index to fub-divide thofe on the circle. This inftrument appears to be made for taking the angle of a ftar at fetting or rifing, or for taking the azimuth or amplitude of the Sun at rifing or fetting.

The ufe of the inftrument, figure D, I was at a lofs to account for. It confifts of two circular walls; the outer of which is about forty feet diameter, and eight feet high; the wall within about half that height, and appears intended for a place to ftand on to obferve the divifions on the upper circle of the outer wall, rather than for any other purpofe; and yet both circles are divided into 360 degrees, each degree being fub-divided into twenty leffer divifions, the fame as the quadrants. There is a door-way to pafs into the inner circle, and a pillar in the center, of the fame height with the lower circle, having a hole in it, being the center of both circles, and feems to be a focket for an iron rod to be placed perpendicular into it. The divifions on thefe, as well as all the other inftruments, will bear a nice examination with a pair of compaffes.

Figure E is a ſmaller equinoctial Sun-dial, conſtructed upon the ſame principle as the large one A.

I cannot quit this ſubject without obſerving, that the Bramins, without the aſſiſtance of optical glaſſes, had nevertheleſs an advantage unexperienced by the obſervers of the more Northern climates. The ſerenity and clearneſs of the atmoſphere in the night-time in the Eaſt Indies, except at the ſeaſons of changing the monſoons or periodical winds, is difficult to expreſs to thoſe who have not ſeen it, becauſe we have nothing in compariſon to form our ideas upon: it is clear to perfection, a total quietude ſubſiſts, ſcarcely a cloud to be ſeen; and the light of the heavens, by the numerous appearance of the ſtars, affords a proſpect both of wonder and contemplation.

This obſervatory at Benares is ſaid to have been built by the order of the emperor ACKBAR; for as this wiſe prince endeavoured to improve the arts, ſo he wiſhed alſo to recover the ſciences of Hindoſtan, and therefore directed that three ſuch places ſhould be erected; one at Delhi, another at Agra, and the third at Benares.

Some doubts have ariſen with regard to the certainty of the ancient Bramins having a knowledge in aſtronomy, and whether the Perſians might not have introduced it into Hindoſtan when conquered by that people; but

but these doubts I think must vanish, when we know that the present Bramins pronounce, from the records and tables which have been handed down to them by their forefathers, the approach of the eclipses of the Sun and Moon, and regularly as they advance give timely information to the emperor and the princes in whose dominion they reside. There are yet some remains in evidence of their being at one time in possession of this science. The signs of the zodiac, in some of their Choultrys on the coast of Coromandel, as remarked by JOHN CALL, esq. F. R. S. in his letter to the Astronomer Royal, requires little other confirmation. Mr. CALL says, that as he was laying on his back, resting himself in the heat of the day, in a Choultry at Verdapetah in the Madura country, near Cape Commorin, he discovered the signs of the zodiac on the cieling of the Choultry: that he found one, equally compleat, which was on the cieling of a temple, in the middle of a Tank before the pagoda Teppecolum near Mindurah; and that he had often met with several parts in detached pieces. See Philos. Trans. 1772, p. 353. These buildings and temples were the places of residence and worship of the original Bramins, and bear the marks of great antiquity, having perhaps been built before the Persian conquest. Besides, when we know that the manners and customs of the Gentoo

religion

religion are ſuch as to preclude them from admitting the ſmalleſt innovation in their inſtitutions; when we alſo know that their faſhion in dreſs, and the mode of their living, have not received the leaſt variation from the earlieſt account we have of them; it cannot be ſuppoſed they would engrave the ſymbolical figures of the Perſian aſtronomy in their ſacred temples; the ſigns of the zodiac muſt therefore have originated with them, if we credit their tradition of the purity of their religion and cuſtoms.

Mr. FRASER, in his Hiſtory of the Mogul Emperors, ſpeaking of time ſays, "the Lunar year they reckon "354 days, 22 gurris, 1 pull; the Solar year they "reckon 365 days, 15 gurris, 30 pulls, $22\frac{1}{2}$ peels; 60 "peels making 1 pull, 60 pulls 1 gurri, and 60 gurris "1 day. This is according to the Bramins or Indian "prieſts, and what the Moguls and other Mahomme-"dans in India chiefly go by."

Thus far Mr. FRASER; and it ſerves to ſtrengthen the argument for ſuppoſing that the Bramins had a knowledge of aſtronomy before the introduction of Mahometaniſm into Hindoſtan.

Dimenſions

Dimenſions of the larger equinoctial Sun-dial, plates 13. and 14.

	Feet.	In.
Length of the gnomon at the baſe *b b*,	34	8
Oblique length of the gnomon *c c*, —	38	8
Radius of the quadrants *a a*, — —	9	2
Height of the gnomon at *d*, — —	22	3
Breadth of the quadrants *f f*, — —	5	10
Thickneſs *g g*, — — —	1	0
Breadth of the gnomon *h h*, — —	4	6
Whole extent of the inſtrument *i i*, —	37	4

Latitude of the place taken by double altitude 25° 10′.

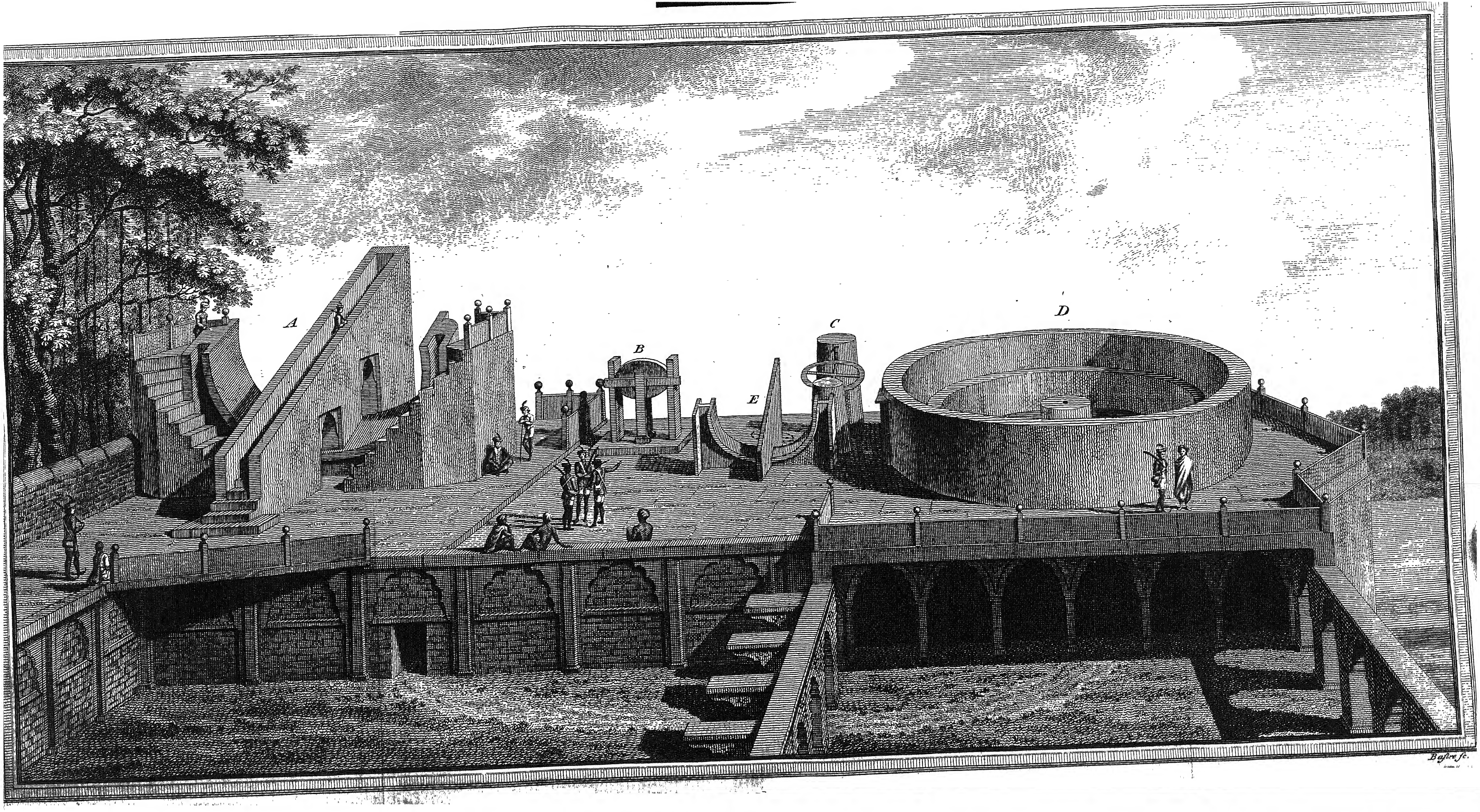
I am, &c.

XXXI. *A ſhort Account of Dr.* Maty's *Illneſs, and of the appearances in the dead Body, which was examined on the 3d of* July, 1776, *the Day after his Deceaſe. By Dr.* Hunter *and Mr.* Henry Watſon, *FF. R. S.*

Read May 1, 1777.

ABOUT two weeks before he died, he was taken with a fit of violent oppreſſive pain, juſt above the pit of the ſtomach, which made him feel as if he was very near dying. He was bled, and gradually recovered; yet ſo imperfectly, that any motion of his body, or any preſſure upon that part with the point of a finger, inſtantly brought on ſuch oppreſſive pain, that he was convinced the leaſt addition to what he had ſeveral times felt, muſt have put an end to his life. He had an idea that there might be a collection of matter behind the ſternum, which might be diſcharged by ſome chirurgical operation.

Upon examining the part, which with the whole body was very much emaciated, there was no protruſion or diſcolouration. All thoughts of making any perforation were laid aſide; and it was thought probable, that there was ſome inflammation or adheſion of the pericardium


A
B
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E
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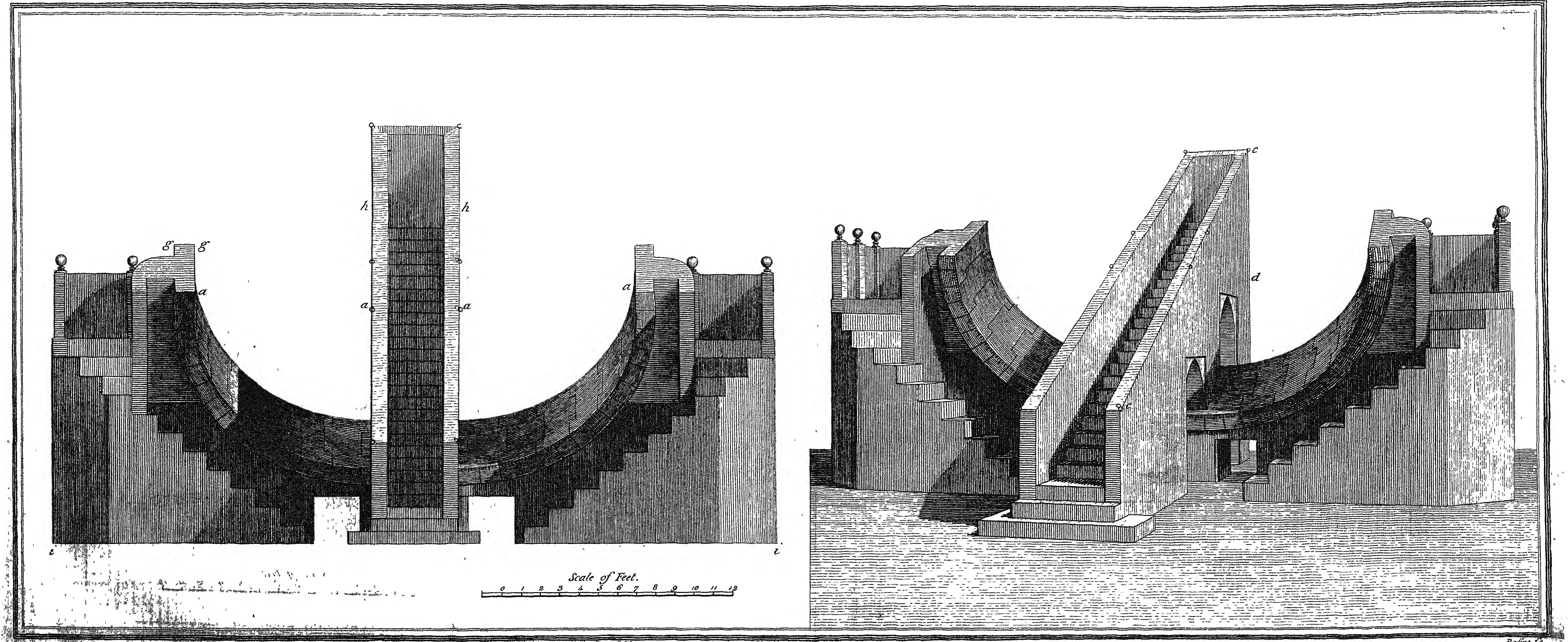

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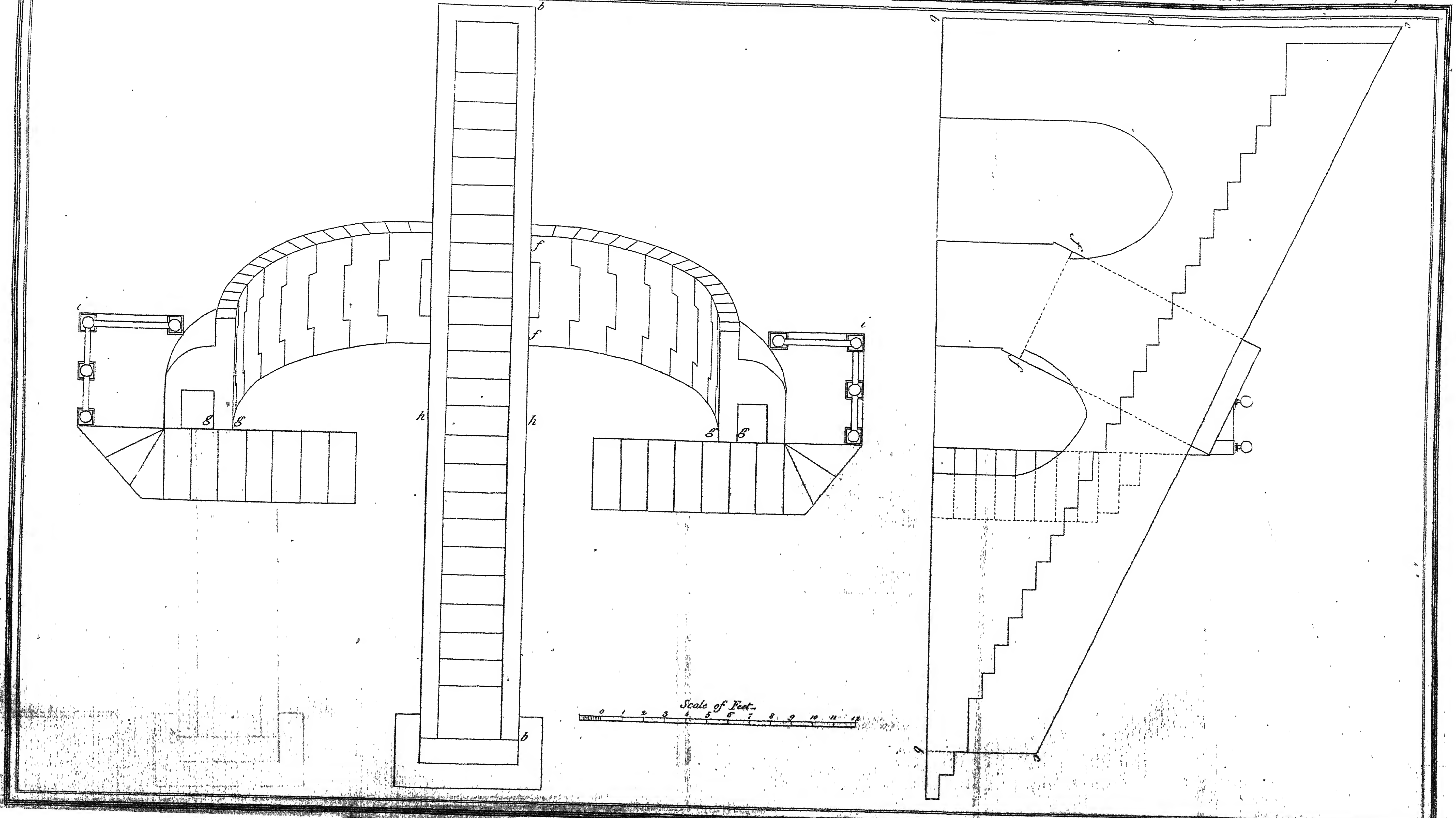
b
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c
Scale of Feet.
0 1 2 3 4 5 6 7 8 9 10 11 12

dium, or of the heart itſelf, at its anterior part, juſt above the diaphragm. His cough was almoſt inceſſant in the night ſince he had left off the uſe of opium, to which he had been long accuſtomed. For ſeven or eight years, he ſaid, he believed he might have had twenty purging ſtools in every twenty-four hours, from a complaint in his bowels, the principal ſeat of which he pointed out ſo exactly in his emaciated ſtate, that it was obſerved at the time it muſt be in the colon, where it paſſes down on the outſide of the lower end of the left kidney. It was therefore thought probable that there was contraction with internal ulceration of the gut at that place: and about three years ago, with this complaint, which always continued in his bowels and left ſide, he had a fiſtula in ano, for which he was cut, and thereby cured of that diſorder; but from that time, he was always ſenſible that the lower part of the rectum remained in an awkward, uneaſy ſtate, ſo that it was difficult and painful to paſs a common gliſter-pipe into it.

His medical friends were of opinion, that no more could be done for him than to palliate, and to procure eaſe and ſleep. He returned to his opium, of which he took one grain twice a day; and at times was thereby much relieved and comforted.

The heart and lungs were examined with great care, but there was hardly any appearance of diſorder in either, contrary to what was expected.

The conjecture that had been formed about the complaint in the bowels proved to be perfectly juſt. The ſmall inteſtines were apparently pretty ſound; the cæcum and beginning of the colon were much diſtended with air, but not inflamed. The arch, or tranſverſe turn of the colon, was likewiſe much diſtended, and its blood-veſſels were ſo loaded, that there was, at firſt ſight, the outward appearance of an internal inflammation. The enlarged part of the colon terminated at the lower end of the left kidney, where there was an annular ſtricture on the outſide of the gut, and there the gut felt hard and fleſhy. The enlarged part being ſlit up, was much inflamed and ſuperficially ulcerated on the inſide, and more in proportion towards the lower end. At the ſtricture there was but a very ſmall paſſage left, winding irregularly through an inch and an half of hard ulcerated gut. Below this, where the colon paſſes over the pſoas and iliac veſſels, it was in its natural ſtate; but the rectum had been at ſome former time very much diſeaſed, and for a finger's length to within two inches of the anus was contracted to almoſt a gooſe-quill ſize, and of a livid colour. The lower two inches were not ſo much contracted,

tracted, but of the ſame livid colour, and the ſurface of the gut there was almoſt as unequal as the faſciculated ſurfaces in the heart; the effect, probably, of univerſal ulceration there, which had been a part of, or a companion to, the fiſtula, of which he had been cured by the operation; for, on that part, the villous coat of the inteſtine was deſtroyed.

TO this account, more particularly of the two laſt weeks of Dr. MATY's illneſs, and of the appearances upon opening the body, as drawn up by Dr. HUNTER, I ſhall beg leave to add the few following remarks.

The heart and lungs were indeed neither of them eſſentially diſeaſed; yet there was a whitiſh ſpot, about the breadth of a ſix-pence, upon the right ventricle of the heart, near its apex; a rough border on the left ſide of the diaphragm, as if the lungs had been glued to that part and torn off again; a partial adheſion of the lungs to the pleura; and a little purulent fluid within the pericardium. Certainly theſe were ſome ſigns of a ſlight inflammation having attacked the membranes inveſting the contents of the thorax. Neither can we ſuppoſe ſuch appearances to have exiſted without occaſioning ſome uneaſineſs: they were, perhaps, ſufficient to account for

that great tendernefs and oppreffive pain which the doctor felt from the leaft preffure on the fternum, or upon any part of the breaft near it.

The principal feat of the difeafe which proved fo tedious, and in the end fo fatal, was, no doubt, confined to the colon only; and it was entirely within the gut. The part firft affected muft have been that portion of the canal in which we obferved the moft mifchief. The fuperficial extent of the difeafe over fo large a furface as the whole arch of the colon, and the more formidable appearance of it, in only a few inches of the fame gut, diftinguifhed the part where the difeafe firft began, and where it muft have had its longeft duration.

The caufe of all this mifchief was conjectural with Dr. MATY himfelf. Had it arifen, as he fufpected, from having bruifed his fide with the hilt of his fword, we then fhould have found the gut injured from without inwards. But is it not moft likely, that a little bit of bone, the ftone of fruit, fome fharp or hard body, in paffing, had injured the gut fo much, as to lay a foundation for all the growing complaints? Nearly the fame appearances have been obferved in the œfophagus from only a hard cruft of bread lodging for a time in the paffage; which, after being forced down, was fucceeded by great forenefs, inflammation, ulceration, and at length

5 fo

ſo complete an obſtruction, as to occaſion the death of the patient; of which I once ſaw a very deplorable inſtance.

The ulcerated inteſtine is a diſeaſe generally, as in the caſe before us, ſlow in its progreſs, but certainly fatal. An accumulation of acrid matter, confined air, ſolid ingeſta, in ſhort any thing capable of ſtretching, irritating, or hardening the gut, will ſpread and increaſe the diſeaſe.

The faſciculated appearance in the rectum is what I have once met with in a very ſound gut, where the villous coat was not in the leaſt injured; it is therefore ſometimes an original conformation, but apparently unneceſſary, as the gut, we may preſume, would perform its office much more agreeably without it.

H. WATSON.

XXXII. *An Account of ſome Experiments made with an Air-pump on Mr.* Smeaton's *Principle; together with ſome Experiments with a common Air-pump. By Mr.* Edward Nairne, *F. R. S.*

Read June 12, 1777.

AS the following experiments were made principally to try the performance of Mr. SMEATON's pear-gage, it may be proper to deſcribe it, which I ſhall do in his own words, taken from the Phil. Tranſ. for the years 1751 and 1752, vol. XLVII. p. 420.

" I have found," ſays Mr. SMEATON, " the gages that " have been hitherto made uſe of, for meaſuring the " expanſion of the air, very unfit to determine in an ex- " periment of ſo much nicety; I have therefore con- " trived one of a different ſort, which meaſures the ex- " panſion with certainty to much leſs than the 1000th " part of the whole. It conſiſts of a bulb of glaſs, ſome- " thing in the ſhape of a pear, and ſufficient to hold " about

"about half a pound of quickſilver: it is open at one
"end, and at the other is a tube, hermetically cloſed
"at the top. By the help of a nice pair of ſcales I found
"what proportion of weight a column of quickſilver, of
"a certain length, contained in the tube, bore to that
"which filled the whole veſſel: by theſe means I was
"enabled to mark diviſions upon the tube anſwering to
"a 1000th part of the whole capacity; which being
"about one-tenth of an inch each, may, by eſtimation,
"be eaſily ſub-divided into ſmaller parts. This gage,
"during the exhauſting of the receiver, is ſuſpended
"therein by a ſlip wire. When the pump is worked as
"much as ſhall be thought neceſſary, the gage is puſhed
"down till the open end is immerged in a ciſtern of
"quickſilver placed underneath; the air being then let
"in, the quickſilver will be driven into the gage till the
"air remaining in it becomes of the ſame denſity with
"the external, and as the air always takes the higheſt
"place, the tube being uppermoſt, the expanſion will be
"determined by the number of diviſions occupied by
"the air at the top.

"The degree to which I have been able to rarify the
"air in an experiment, has generally been about 1000
"times, when the pump is put clean together; but the
"moiſture

"moiſture that adheres to the inſide of the barrel as "well as other internal parts, upon letting in the air, is "in the ſame ſucceeding trials worked together with the "oil, which ſoon renders it ſo clammy as to obſtruct "the actions of the pump upon a fluid ſo ſubtil as the "air when ſo much expanded; but in this caſe it ſeldom "fails to act upon the air in the receiver till it is ex-"panded 500 times." Thus far Mr. SMEATON'S account.

The pump with which the chief of the following experiments were made, had the leather of its piſton ſoaked in oil and tallow (and oil in the barrel) and every precaution was taken that no water ſhould get into the working parts of the pump, except what might ariſe in vapour from the ſubſtances which were under the receiver.

EXPERIMENT I.

Having provided a pear-gage agreeable to Mr. SMEATON'S deſcription, on which the ſpace of a 4000th part of the whole capacity was two-tenths of an inch; this gage, together with a glaſs cup which ſerved as a ciſtern

to

to hold the quickſilver for it, was put under a receiver; which receiver was placed on a leather ſoaked in oil and tallow, on the plate of the pump.

I muſt obſerve here, that the foot of the glaſs cup, which held the quickſilver for the pear-gage, being broken off by an accident, another foot, made of a piece of box-wood, was cemented to it. The reaſon of mentioning this circumſtance, which may ſeem trifling, will appear in the ſixth experiment. The pump was then worked, and the pear-gage was puſhed down till its open end was immerſed in the quickſilver in the cup: the air being then let into the receiver, it forced the quickſilver into the gage till it was filled within a 4000th part of the whole, ſo that by this gage the pump appeared to have expanded the air 4000 times. To what it might be owing that this pump, which was on Mr. SMEATON's principle, ſhould expand the air 4000 times inſtead of only 1000 as Mr. SMEATON's, I could not even ſurmiſe.

Having the pleaſure of knowing Mr. SMEATON, and being well acquainted with his great abilities in practice as well as theory, I could not imagine that this apparent ſuperiority could proceed from our having executed the various parts of the pump in a more perfect manner than he

he had done. I therefore determined, for greater certainty, to ſee if the fault might not be in the gages; and for that purpoſe I repeated the experiment with the ſyphon-gage, and both the long and ſhort barometer-gages, and found that the ſeveral degrees of exhauſtion indicated by theſe, were very different from that which the pear-gage had indicated: no concluſion, therefore, could be drawn from this experiment.

I determined next to compare again this pear-gage with the long and ſhort barometer-gages with all the accuracy I was maſter of: and firſt, leſt the tubes of theſe barometer-gages might not be perfectly clean and free from moiſture, I had ſome tubes made at the glaſs houſe; and as ſoon as they were brought home, which was within an hour after they were made, two of them, which were of the ſame ſize, were filled with diſtilled quickſilver; and then the quickſilver was carefully boiled in the tubes the whole length, which was about thirty-ſix inches: I then cut off about ſix inches from the ſealed end of one of the tubes, and took care to keep it perfectly full of the boiled quickſilver; it was then inverted into a glaſs ciſtern containing boiled quickſilver; and a piece of very thin ivory, about half an inch in length, with diviſions on its edge, was put over the tube, ſo as to float on the ſurface

of

of the quickſilver in the ciſtern, by which means the difference of the two ſurfaces could be ſeen to a great nicety. This kind of gage is called the ſhort barometer-gage.

The other tube, which was cut off to thirty-three inches, being perfectly full of the boiled quickſilver, was alſo carefully inverted into a glaſs ciſtern containing boiled quickſilver, to ſuch a depth that from the ſurface of the quickſilver in the ciſtern to the top of the tube was twenty-nine inches; this had likewiſe a piece of ivory, with diviſions on it, put over the tube, ſo as to float on the ſurface of the quickſilver in the ciſtern, in the ſame manner as the other.

This long tube and the ſhort barometer-gage being put both of them at the ſame time under the receiver, which was carefully cemented to the plate of the pump; the pump was then worked for ten minutes, and the ſurface of the quickſilver in both the tubes came down very nearly to within one-twentieth or five hundredth parts of an inch of the ſurface of the quickſilver in their reſpective ciſterns.

The air was then let in, and the receiver being taken from the pump, the long tube was raiſed up ſo far in the ciſtern as to let the quickſilver come down from the top of the tube, ſo that it now became a common barometer, and

and its height from the ſurface of the quickſilver in the ciſtern meaſured thirty inches; which agreed exactly with an open ciſtern-barometer I had in the room. The quickſilver in the tube of this barometer was alſo boiled, and the meaſure the ſame.

The quickſilver was then emptied out of this long tube, and the ſealed end being cut off, it was then cemented to a piece of braſs, by which means it was ſcrewed to the air-pump; and the lower end being immerſed in a cup of boiled quickſilver, it then made that kind of gage where the air is taken from the top of the tube, and which is called the long barometer-gage. This gage being fixed to the pump, and the ſhort barometer-gage put on the plate of the pump under a receiver, the receiver was cemented to the plate of the pump, and the pump worked for ten minutes as before. The quickſilver in the ſhort barometer-gage fell now nearly to within one-twentieth or five hundredth parts of an inch of that in the ciſtern, and the quickſilver in the long gage roſe nearly to within a twentieth or five hundredth parts of an inch of the height it was at when it was made as a common barometer.

Gages made with theſe precautions ſeem to me to be the moſt to be depended upon, in determining the actual

diminution

diminution of the preſſure on the ſurface of the quickſilver in the tube of the long gage, and alſo on the ſurface of the quickſilver in the ciſtern of the ſhort gage. But of theſe two gages the long barometer-gage was chiefly uſed in the following experiments, as being fixed to the pump: however, having now made theſe two gages with as much accuracy as I was maſter of, and finding that they agreed pretty nearly, I proceeded to repeat my firſt experiment.

EXPERIMENT II.

I put the ſhort barometer-gage, and the pear-gage with the glaſs cup having a wooden foot, both together under the receiver, which receiver was placed on a leather ſoaked in oil and tallow on the plate of the pump; the pump was then worked for ten minutes, and the quickſilver was brought down in the ſhort barometer-gage to about one-tenth of an inch of the ſurface of the quickſilver in the ciſtern, and roſe in the long barometer-gage to within one-tenth of an inch of the height of the quickſilver in my ſtandard barometer, which was at that time at thirty inches; by which it appeared, that

the preſſure on the ſurface of the quickſilver in the ciſtern, and in the tube of the long barometer-gage, was diminiſhed to about a three-hundredth part: the pear-gage being now puſhed down till its open end was immerſed under the ſurface of the quickſilver in the cup, the air was then let in, and the pump appeared by that gage to have exhauſted all but a ſix thouſandth part of the air; or, in other words, the degree of exhauſtion by this gage appeared to be ſix thouſand times.

Finding ſtill this diſagreement between the pear-gage and the other gages, I tried a variety of experiments; but none of them appeared to me ſatisfactory, till one day in April 1776, ſhewing an experiment with one of theſe pumps to the honourable HENRY CAVENDISH, Mr. SMEATON, and ſeveral other gentlemen of the Royal Society, when the two gages differed ſome thouſand times from one another, Mr. CAVENDISH accounted for it in the following manner. "It appeared," he ſaid, "from ſome experiments of his father's, Lord CHARLES "CAVENDISH, that water, whenever the preſſure of the "atmoſphere on it is diminiſhed to a certain degree, is "immediately turned into vapour, and is as immediately "turned back again into water on reſtoring the preſ-"ſure. This degree of preſſure is different according to "the heat of the water: when the heat is 72° of FAH-

"RENHEIT'S

" RENHEIT's ſcale, it turns into vapour as ſoon as the
" preſſure is no greater than that of three quarters of an
" inch of quickſilver, or about one-fortieth of the uſual
" preſſure of the atmoſphere; but when the heat is only
" 41°, the preſſure muſt be reduced to that of a quarter
" of an inch of quickſilver before the water turns into
" vapour. It is true, that water expoſed to the open air
" will evaporate at any heat, and with any preſſure of
" the atmoſphere; but that evaporation is intirely owing
" to the action of the air upon it: whereas the evapora-
" tion here ſpoken of is performed without any aſſiſtance
" from the air. Hence it follows, that when the receiver
" is exhauſted to the above-mentioned degree, the moiſ-
" ture adhering to the different parts of the machine
" will turn into vapour and ſupply the place of the air,
" which is continually drawn away by the working of
" the pump, ſo that the fluid in the pear-gage, as well as
" that in the receiver, will conſiſt in good meaſure of va-
" pour. Now letting the air into the receiver, all the
" vapour within the pear-gage will be reduced to water,
" and only the real air will remain uncondenſed; conſe-
" quently the pear-gage ſhews only how much real air
" is left in the receiver, and not how much the preſſure
" or ſpring of the included fluid is diminiſhed, whereas

" the

"the common gages ſhew how much the preſſure of "the included fluid is diminiſhed, and that equally, "whether it conſiſt of air or of vapour."

Mr. CAVENDISH having explained ſo ſatisfactorily the cauſe of the diſagreement between the two gages, I conſidered, that, if I were to avoid moiſture as much as poſſible, the two gages ſhould nearly agree: this induced me to make the following experiment.

EXPERIMENT III.

The plate of the pump being made as clean and as dry as poſſible, there was then put on it the before-mentioned ſhort barometer-gage, alſo the pear-gage with a ciſtern entirely of glaſs which held the quickſilver; they were then covered with a receiver, round the outſide of which was laid a cement which perfectly excluded the outward air; every part, before it was put under the receiver, as well as the receiver itſelf, being made as clean and as free from moiſture as poſſible[(a)]. The pump was then worked for ten minutes,

(a) It may be proper here to take notice, that the pump in every experiment hereafter mentioned was worked ten minutes, and the ſame receiver continued cemented to the pump-plate, except where it is otherwiſe mentioned. The top part of this receiver was made to open, in order to put in different things.

and the barometer-gages indicated a degree of exhauſtion nearly 600; the air was then let into the receiver, the pear-gage indicated a degree of exhauſtion, but very little more than 600 alſo. The near agreement of the pear-gage with the barometer-gages in this laſt experiment, in which I had been ſo careful to exclude the moiſture as much as poſſible, ſeemed to prove beyond a doubt, that their diſagreeing in the firſt and ſecond experiments muſt have been owing (as Mr. CAVENDISH ſuppoſed) to the moiſture which in them had not been ſo carefully excluded. But I began now to ſuſpect alſo, that there might ariſe a vapour from ſome moiſture that might be contained in the leather ſoaked in oil and tallow, or in the wooden foot which was cemented to the glaſs cup, both uſed in the firſt and ſecond experiments: theſe ſuſpicions induced me to try the following experiments.

EXPERIMENT IV.

A piece of leather dreſſed in allum, known by the name of white ſheep-ſkin, of about four inches diameter, which had been ſoaked in oil and tallow about a year ago (ſuch as was uſed to place the receiver on in the firſt and ſecond experiments) was put into the receiver; the

pump was then worked, and the barometer-gage indicated a degree of exhaustion of nearly 300; but on the admission of the air the pear-gage indicated a degree of exhaustion of 4000.

EXPERIMENT V.

The piece of leather being taken out, the pump was then worked, and the degree of exhaustion appeared by both the barometer and pear-gages to be about 600, as in the third experiment.

EXPERIMENT VI.

A cylinder made of a piece of box wood (which I had kept by me for more than a year) one inch in diameter and three inches in length, was put into the receiver (this piece of wood was of the same kind as that which was cemented to the foot of the glass cup used in the first and second experiments) the pump was then worked, and the degree of exhaustion appeared by the barometer-gage to be 300, but by the pear-gage 16,000.

These experiments have often been repeated, but the result was seldom the same. When leather soaked in oil and tallow has been put into the receiver, the pear-gage has sometimes indicated a degree of exhaustion of

20,000,

20,000, and sometimes no more than 500; it likewise differs very much with the box wood, which may perhaps be owing to different degrees of heat and moisture.

From these experiments it is evident, that there arises an elastic vapour from the leather dressed in allum and soaked in oil and tallow, and also from the piece of box wood, when the weight of the atmosphere has been partly taken off by the action of the pump; and that this vapour presses upon the surface of the quicksilver in the tube of the long barometer-gage, and of that in the cistern of the short one; and that, consequently, the testimony of both these gages must be influenced by this vapour, as well as by the small remainder of common air: but as it is the nature of the pear-gage not to give its testimony till the remaining air contained in it is pressed, so as to become of the same density of the atmosphere; and as this vapour cannot subsist in the form of vapour under that pressure, this gage is not at all influenced by it, but indicates the remaining quantity of permanent air only.

Seeing thus what a considerable quantity of vapour arose from the compound of leather, allum, oil, and tallow, my next object was to find out from which of those substances it chiefly arose; how far I have succeeded will appear by the following experiments.

Subftances put into the receiver.	Weight when put into the receiver.	Degree of exhauftion according to Barom. gage.	Pear-gage.
EXP. VII. Tallow, — —	2 ounces	431	600
EXP. VIII. Oil, — —	2 ounces	377	480
EXP. IX. Allum, — —	2 ounces	378	580
EXP. X. A piece of leather as it came from the leather-fellers, — — —	100 grains	152	100,000.
EXP. XI. The fame piece of leather foaked in the fame two ounces of tallow and oil melted together, — —		432	800

From thefe experiments it appears, that the elaftic vapour which caufed fo great a difference in the teftimony of the gages, arofe principally from the leather, and but little from the tallow, oil, or allum: it even appears by the tenth experiment, that it came from the leather, and fupplied the place of the exhaufted air fo faft, that I could not (at leaft in the ten minutes) make the barometer-gage indicate a degree of exhauftion of of more than 159.

To determine whether it was the moifture in the leather from which the vapour arofe, I made the following experiments.

Sub-

Subſtances put into the receiver.	Weight when put into the receiver.	Degrees of exhauſtion according to		Variation in weight during the experiments.
		Barom. gage.	Pear-gage.	
EXP. XII. A piece of white leather, freſh from the leather-ſellers, — —	100 grains	134	100,000	loſt 2 grains.
EXP. XIII. The ſame piece of leather, dried by the fire till it would loſe no more of its weight, — —	80 grains	268	280	gained 2 grs.
EXP. XIV. The ſame piece of leather held in the ſteam of hot water till it had regained the 20 grains it had been deprived of, — —	100 grains	147	100,000	loſt 2 grains.

In this laſt experiment it was full three quarters of an hour before the leather regained the twenty grains of weight, although it was held very near the ſurface of the hot water.

Whenever I have aſſerted, that the degree of exhauſtion, according to the pear-gage, was ſo great as 100,000, I only gueſſed it to be thereabouts, for my gage is not graduated to more than 4000; but, that it may be ſeen what reaſon I had to ſuppoſe it at 100,000, I have brought my pear-gage filled in this laſt experiment, for the inſpection of the gentlemen preſent.

EXPERIMENT XV.

The ſame piece of leather uſed in the eleventh experiment was put into a damp cellar, where it was left till the

the next day; it was then put again into the receiver, and the degree of exhauſtion, according to the barometer-gage, was 300, and according to the pear-gage 3500.

Being now perfectly ſatisfied that the variation in the teſtimony of the pear and barometer-gages was occaſioned by the moiſture contained in the ſubſtances I had put into the receiver aſſuming the form of vapour; I determined next to try what would be the effect of the vapour which might ariſe from ſmall quantities of different fluids, and from ſome other ſubſtances containing moiſture of various kinds.

Subſtances put into the receiver.	Weight when put in.	Degree of exhauſting according to		Change in weight during the experiment.
		Barom. gage.	Pear-gage.	
EXP. XVI. Water in a watch-glaſs,	3 grains	148	24,000	loſt 1½ grain.
EXP. XVII. Water in a glaſs cup, diameter two inches, —	100 grains	89	8000	loſt 2 grains.
EXP. XVIII. Spirit of wine in the ſame cup, —	100 grains	54	6000	loſt 9 grains.
EXP. XIX. Vitriolic acid, —	100 grains	340	220	gained 1 gr.
EXP. XX. A piece of the inſide of a china orange with ſome of the rind, —	100 grains	160	100,000	loſt 2½ grs.
EXP. XXI. A piece of the inſide of an onion, —	100 grains	160	100,000	loſt 1½ grain.
EXP. XXII. A piece of tainted beef,	100 grains	152	100,000	loſt 2½ grs.
EXP. XXIII. A piece of freſh beef,	100 grains	136	100,000	loſt 2½ grs.
EXP. XXIV. Spirit of turpentine,	100 grains	301	1800	loſt 2 grains.
EXP. XXV. Pearl-aſh, —	2 ounces.	118	5000	
EXP. XXVI. The ſame pearl-aſh made very hot, —		198	420	
EXP. XXVII. A lighted candle held in the receiver till it went out, — —		297	1800	

Sub-

Subſtances put into the receiver.	Weight when put in.	Degrees of exhauſting according to		Change in weight during the experiment.
		Barom. gage.	Pear-gage.	
EXP. XXVIII. A piece of charcoal,		129	1800	
EXP. XXIX. The receiver heated by holding ſeveral pieces of lighted charcoal in it, and then the above piece being thoroughly lighted was put into the receiver, and the pump worked, —		650	600	
EXP. XXX. Camphire, —	100 grains	304	520	loſt barely ½ a grain.
EXP. XXXI. Sulphur made to burn on a piece of braſs, —		247	320	

Obſerving by theſe experiments that the ſmall quantity of moiſture which exhaled from the ſubſtances under the receiver prevented the pump from exhauſting it to any very conſiderable degree, I began to ſuſpect that whenever wet leather had been uſed to connect the receiver with the plate, there muſt have riſen ſo great a quantity of vapour as to have prevented the degree of exhauſtion from being near ſo great as in ſome of the foregoing inſtances. Theſe ſuſpicions induced me to make the following experiments.

EXP.

	Degrees of exhaustion according to	
	Barom. gage.	Pear-gage.
EXP. XXXII. The receiver was taken off, and after the cement was wiped clean from it, and every part made perfectly dry, it was put again on the pump-plate, and a little oil only was poured round the outside edge,	nearly 600	full 600
EXP. XXXIII. The receiver was taken off again, and instead of the oil it was set on a piece of leather, which had been soaked two days in water, —	51	16,000
EXP. XXXIV. The last experiment repeated with the same piece of leather, — —	51	1500
EXP. XXXV. The last experiment repeated again with the same piece of leather, — —	51	1000
EXP. XXXVI. The receiver was taken off, and instead of the leather soaked in water, there was put on a piece of the same sort of leather, soaked in a mixture of water and spirit of wine, such as Mr. SMEATON used,	47	12,000
EXP. XXXVII. The last experiment repeated with the same leather, — — —	47	1150
EXP. XXXVIII. The last experiment repeated again with the same leather, — —	47	500

The great difference in the testimony of the pear-gage in these six last experiments appeared to me exceedingly astonishing, for the leathers seemed each of them to be as moist at last as at first.

By these experiments I was convinced how effectually the use of leather soaked in water, or in water and spirit of wine, prevents the pump from exhausting to any considerable degree. I have made a number of experiments of the same kind as these; but have never been able to exhaust, under such circumstances, to a greater degree than between 50 and 60, when the heat of the room was

about 57° by a thermometer of FAHRENHEIT's ſcale: but the following experiments will ſhew how much ſome different degrees of heat affect the degree of exhauſtion.

	Height of the Therm.	Degrees of exhauſtion according to Barom. gage.	Degrees of exhauſtion according to Pear-gage.
EXP. XXXIX. Receiver ſet on leather which had lain all night in water, —	46	84	20,000
EXP. XL. Receiver ſet on a leather ſoaked all night in two parts water and one of ſpirit of wine, — — —	46	76	8000

The pump having been put in a room of the heat of 57° of FAHRENHEIT's ſcale for ſeven hours together, with the leathers put in the ſame water and the ſame ſpirit of wine and water which they had been ſoaked in all night, and which had been uſed in the two laſt experiments, the following experiments were made.

	Height of the Therm.	Degree of exhauſtion according to Barom. gage.	Degree of exhauſtion according to Pear-gage.
EXP. XLI. The receiver ſet on the leather ſoaked in water, — —	57	56	16,000
EXP. XLII. Receiver placed on a leather ſoaked in water and ſpirit of wine, —	57	49	1200

The following table will ſhew the comparative excellency between the pump on Mr. SMEATON's principle with which the chief of theſe experiments have been tried,

tried, and one of my common double barreled table air-pumps under the ſame circumſtances. The leather on the piſtons of both was ſoaked in oil and tallow, and the receiver cemented down to each plate; the pumps were both of them freſh oiled.

	Pump on Mr. SMEATON's principle. Degrees of exhauſtion according to		Common pump. Degrees of exhauſtion according to	
	Barom. gage.	Pear-gage.	Barom. gage.	Pear-gage.
EXP. XLIII. A piece of leather, weighing 100 grains, as it came from the leather-ſellers, was put into the receiver of each pump, both pieces being cut from the ſame ſkin cloſe by each other, —	152	100,000	108	12,000
EXP. XLIV. The ſame pieces of leather dried by the fire till they would loſe no more of their weight, — —	506	520	160	165

The following experiments will ſhew the effect of water uſed in the barrels of pumps to make the piſtons move air tight in them.

I took the ſame common air-pump uſed in the laſt experiment, and having taken off the leathers ſoaked in oil and tallow from the piſtons of this pump, and wiped the barrels as clean as poſſible, I then put new leathers, which had been ſoaked in water, and new bladder valves; the receiver was then cemented to the pump-plate as before.

	Degrees of exhaustion according to Barom. gage.	Degrees of exhaustion according to Pear-gage.
EXP. XLV. The pump was then worked as usual, —	37	38
EXP. XLVI. The last experiment repeated with another common pump, the leathers of the pistons of which were also soaked in water, — —	34	37

From these experiments it evidently appears, that the air-pump of OTTO GUERICKE, and those contrived by Mr. GRATORIX, and Dr. HOOKE, and the improved one by Mr. PAPPIN, both used by Mr. BOYLE, also HAUKSBEE's, S'GRAVESANDE's, MUCHENBROOK's, and those of all who have used water in the barrels of their pumps, could never have exhausted to more than between 40 and 50, if the heat of the place was about 57; and although Mr. SMEATON, with his pump, where no water was in the barrel, but where leather soaked in a mixture of water and spirit of wine was used to set the receiver on the pump-plate, may have exhausted all but a thousandth or even a ten thousandth part of the common air, according to the testimony of his pear-gage; yet so much vapour must have arisen from the wet leather, that the contents of the receiver could never be less than a seventieth or eightieth part of the density of the atmosphere: nevertheless, it does not seem that any deficiency in the construction of Mr. SMEATON's pump was the cause of his

not being able to exhauſt beyond the low degrees of 70 or 80. Had he been aware of the bad effects of ſetting the receiver upon leather ſoaked in water and ſpirit of wine; and had he made uſe of the precaution to free all parts of his pump as much as poſſible from moiſture, I make not the leaſt doubt but the air-pump, which he executed himſelf, would have exhauſted to as great a degree, as that pump has been ſeen to have done with which the chief of theſe experiments were made.

Having read the principal part of this paper to Mr. SMEATON, and ſhewn him ſome of the experiments; one in particular, where the pear-gage, as he obſerved himſelf, was filled to no leſs than 100,000th part of the whole content; he remarked from memory, that he had in ſeveral trials exceeded 1000 times, and once, as he remembered, near or about 10,000 times; but as he never could account how this happened, which appeared to him perfectly accidental, and therefore could not depend upon doing it at pleaſure, he contented himſelf with putting down 1000 times, as being what (under the circumſtances mentioned in his papers) he had a tolerable certainty of.

I muſt here again obſerve, that if we only wiſh to know the quantity of permanent air remaining in the receiver after it is as much exhauſted as poſſi-

ble,

ble, it ſeems, that it is by Mr. SMEATON's gage only that we can know it. Again, when by the aſſiſtance of his gage and the barometer-gage together, we have diſcovered that there is a vapour which ariſes and occupies the place of the permanent air which is exhauſted, it ſeems that it is by the means of his gage only that we can diſcover what part of the remaining contents of the receiver conſiſts of this vapour, and what part of permanent air.

An account of ſome further experiments made with the ſame air-pump on Mr. SMEATON'*s principle, the reſults of which were different from the former.*

AFTER I had made the foregoing experiments, and thought to have done with the ſubject (for ſome time at leaſt) in reviewing them for the laſt time, I perceived one or two, the extraordinary reſults of which (though not unnoticed by me before) I now thought I had not paid ſufficient attention to. Experiment the 19th (in which I found that when vitriolic acid was put into the receiver, and the pump worked for the uſual time, the pear-gage indicated a much leſs degree of exhauſtion than the barometer-

barometer-gage) ſeemed to me now ſo ſurprizing, that it was impoſſible not to wiſh to repeat this experiment with all the care poſſible, and to endeavour to recollect all the circumſtances which I thought could any way influence the reſult.

The vitriolic acid I made uſe of in the following experiments was ſome that I had had by me for ſome time; it had been kept in a phial, ſtopped with a glaſs-ſtopper, and tied over with a bladder. The thermometer in the room was at 59°, and the weather remarkably dry.

	Weight when put into the receiver.	Degrees of exhauſtion according to Barom. gage.	Degrees of exhauſtion according to Pear-gage.	Variation of weight during the experiment.
EXP. XLVII. Vitriolic acid in a glaſs cup, two inches diameter,	100 grains	602	380	gained 1 gr.
EXP. XLVIII. The laſt experiment repeated with the ſame vitriolic acid in the ſame cup,	101 grains	502	350	gained ½ a gr.
EXP. XLIX. The former experiment repeated again with the ſame vitriolic acid in the ſame cup, — —	101½ grs.	502	350	
EXP. L. The former experiment repeated the fourth time in every reſpect as before,	101½ grs.	502	340+	gained a ¼ of a grain.

The generation of vapour in the exhauſted receiver which Mr. CAVENDISH had ſuppoſed, and which I ſeemed, by my former experiments, to have proved, appeared ſatisfactorily to have accounted for the pear-

gage's

gage's indicating a greater degree of exhauſtion than the barometer-gage: but what to ſuppoſe could poſſibly make it indicate a leſs, I was entirely at a loſs; for after having made theſe ſurprizing experiments on the vitriolic acid, I wiſhed once more to try if I could repeat, with the ſame reſult, ſome of my former experiments, in which the pear-gage had indicated ſo nearly the ſame degree of exhauſtion as the barometer-gages. Accordingly I took away the vitriolic acid from under the receiver; the pump was then freſh oiled, and I was very careful to wipe clean and dry the receiver and pump-plate, and then cemented down the receiver as uſual.

EXPERIMENT LI.

The pump was then worked, and the degree of exhauſtion appeared by the barometer-gage to be 432; but by the pear-gage to be but 370.

I know of no circumſtance attending this experiment that differed from thoſe in which my former experiments were made when the gages agreed ſo nearly, unleſs it were that of the weather: I recollect that it was then very damp, and now it had been very dry for ſome time. How this circumſtance could make ſo great an alteration in the reſult of theſe experiments, I cannot pre-

tend

tend to ſay; but ſome of the following experiments will ſhew that the pear-gage ſtill continued in many caſes to indicate a leſs degree of exhauſtion than the barometer-gage.

In all the preceding experiments the pump was worked for ten minutes, and the pear-gage was at the end of that time puſhed down ſo as for the mouth to be immerſed in the ciſtern of quickſilver, and the air then let in according to the manner of uſing this gage; but now that I found that the teſtimony of this gage ſo ſeldom agreed with that of the barometer-gage, I wiſhed to try, whether they might not agree when the receiver was exhauſted only in part, though they did not when it was exhauſted as much as poſſible. For this purpoſe I had the ſame receiver fitted with two pear-gages, ſo that I now could immerſe the mouth of one of them in the ciſtern of quickſilver when I had exhauſted the receiver in part only, and not immerge the mouth of the other till the receiver was exhauſted for the uſual time of ten minutes; in which time I found I could always raiſe the quickſilver in the barometer-gage as high as if I were to work the pump much longer.

EXP.

	Degrees of exhauftion when the pump had been worked			
	Five minutes; according to		Ten minutes; according to	
	Barom. gage.	Pear-gage.	Barom. gage.	Pear-gage.
EXP. LII. The receiver cemented to the pump-plate, —	430	300	430	360
EXP. LIII. The receiver uncemented, wiped clean from the cement, and put on the pump-plate, with a little oil round the outfide, —	502	360	502	360
EXP. LIV. The receiver put on a leather foaked in oil and tallow,	502	320	323	500

This laft experiment feemed very extraordinary; for after having worked the pump for five minutes only, the barometer-gage indicated a degree of exhauftion of 502; but by working the pump five minutes more, it indicated a lefs degree of exhauftion, *viz.* 323. This effect I have obferved to happen more than once.

	Degrees of exhauftion when the pump had been worked			
	Five minutes; according to		Ten minutes; according to	
	Barom. gage.	Pear-gage.	Barom. gage.	Pear-gage.
EXP. LV. Receiver put on a leather foaked in water for one night,	47	380	63	8000
EXP. LVI. Receiver put on a leather foaked for one night in a mixture of two parts water and one fpirit of wine, — —	48	300	50	1200

The

The receiver was then taken off from the pump-plate, and a blank ſcrew ſcrewed into the hole in the pump-plate.

EXPERIMENT LVII.

The pump was then worked for ten minutes without any receiver on the pump-plate. The barometer-gage alone being afterwards connected with it, the gage indicated a degree of exhauſtion of 50 only, which was the ſame as in the laſt experiment; ſo that the bad effects of the mixture of the ſpirit of wine and water ſtill continued.

I then poured about two ſpoonfuls of oil down the hole in the pump-plate, and the piſtons were worked gently till moſt of the oil had paſſed through the pump into a reſervoir made to receive it. A blank ſcrew was then ſcrewed into the hole in the pump-plate as before, and after the pump had been worked for a minute or two, four or five times, the air being let in between each time, the oil had then waſhed ſo much of the moiſture out of the inſide of the pump, that I was now able to exhauſt to 430 inſtead of only 50 times, as before the oil had been made to paſs through it.

During the courſe of theſe experiments on the air-pump it appeared, by the teſtimony of the pear and baro-

meter-gages, that the remaining contents of a receiver, when exhauſted as much as poſſible, was at different times of different kinds; ſometimes it ſeemed to conſiſt entirely of permanent air, as when a little vitriolic acid, &c. was put in the receiver; and ſometimes moſtly of vapour ariſing from moiſture, and but a very ſmall proportion of permanent air, as when a bit of damp leather, &c. was in the receiver. I was now therefore deſirous of ſeeing what appearance the electric matter would exhibit in theſe different rarified media.

For this purpoſe I had a glaſs tube made, of an inch bore, and four feet and a half in length. This tube was connected to the receiver of the air-pump by means of an elbow-piece of braſs, to which it was cemented; which elbow-piece was inſerted perpendicularly in the top of the receiver: as the elbow made a right angle, the tube itſelf was of courſe horizontal.

Moreover, at that end of the glaſs tube which was cemented to the braſs elbow-piece, there was fixed, on the inſide, a piece of braſs wire, about three inches and a half long, filed to a point, and pointing towards the other end of the tube. At the other end of the tube was cemented a braſs ſcrew fitted to a braſs cap, which ſcrewed on it; and in this braſs cap was fixed a braſs wire, three inches and a half long, which pointed towards

the braſs point; at the other end of the tube, and at the end of this wire, was a braſs ball, eight-tenths of an inch diameter.

The braſs cap at this extremity of the tube which is fartheſt from the receiver was made round, and placed ſo as to be in contact with the prime conductor of an electrical machine.

I now firſt put ſome vitriolic acid into the receiver, as a means of being able to make the remaining contents of the receiver, when exhauſted as much as poſſible, to conſiſt of permanent air only, unadulterated with vapour; and as the receiver was the ſame I had uſed in my foregoing experiments, there were two pear-gages fitted to it, which pear-gages I puſhed down into the ciſterns of quickſilver at different times, and the pump was worked as in my former experiments for ten minutes: heat of the room 59°.

The electrical machine was worked during the whole of the experiments.

EXPERIMENT LVIII.

Electrical appearances exhibited.	Degrees of exhauſtion according to Barom. gage.	Degrees of exhauſtion according to Pear-gage.
Light began firſt to appear in flaſhes, — —	5	
Light appeared the whole length of the tube in ſtriæ,	8	
Tube was filled with an uniform body of pale light,	74	75
The pump had now been worked five minutes.		
The pump was then worked five minutes more.		
The tube was ſtill filled with a uniform body of pale light, — — — —	269	230

The conductor being then removed to a diſtance from the tube, it was made to approach it by degrees till a ſpark ſtruck it, which was at the diſtance of two inches; the light in the tube now appeared like a compact body of fire, of a vivid purple colour, tending to a red.

Objects were ſeen through the tube when filled with this body of the electrical light, no leſs diſtinctly than if there had been no ſuch light in the tube[a].

The vitriolic acid being taken out of the receiver, I put a piece of leather of 100 grains, as it came from the leather-ſellers, into that end of the tube which was next to the conductor of the electrical machine, and fartheſt from the receiver of the air-pump. I put the leather in-

(a) This circumſtance has been before remarked by Dr. HAMILTON in his conjectures on the tails of comets.

at this end of the tube rather than into the receiver, to be ſure that the tube might be filled with the vapour ariſing from the damp leather rather than with the common air. The pump and electrical machine were then worked as before.

EXPERIMENT LIX.

Electrical appearances.	Degrees of exhauſtion according to Barom. gage.	Degrees of exhauſtion according to Pear-gage.
Light began firſt to appear in flaſhes, — —	12	
Light appeared the whole length of the tube in ſtriæ,	22	
Light vaniſhed ſcarce to be ſeen, — — —	90	100
The pump had now been worked ſeven minutes. The conductor was now removed from the tube, and the greateſt ſtriking diſtance was found to be one inch,		
The tube now appeared luminous, but the light was faint and white. The conductor was then again put in contact with the tube, and the machine worked: the pump was alſo worked for three minutes more, but ſcarce any light appeared, — —	148	20,000

The conductor was then again removed from the tube, and the ſtriking diſtance was found to be only one inch and four-tenths: the tube at the time of ſtriking was luminous as before, and the light was of the ſame faint white colour.

Having lately received from my friend Dr. LIND ſome æther prepared by the ingenious Mr. WOLFE, I was very deſirous to try whether I could produce any conſiderable degree

degree of cold by the evaporation of æther under a receiver whilſt exhauſting. For this purpoſe I put the æther into a phial, the neck of which was ſufficient to admit the ball of a thermometer: this being placed on the air-pump, under a receiver which had a plate at the top, with a wire paſſing through a collar of leathers; to this wire the thermometer was fixed, by which means I could eaſily dip the ball of the thermometer into the æther.

EXPERIMENT LX.

The pump was now worked, and whilſt the receiver was exhauſting, the ball of the thermometer was often dipped into the æther; and when the degree of exhauſtion by the barometer-gage was 65 (which was the utmoſt in this caſe that the pump would exhauſt to) the degree of cold indicated by the fall of the quickſilver in the thermometer was 48° below 0 on FAHRENHEIT'S ſcale; ſo that there was a degree of cold produced 103° colder than the air in the room where the experiment was made, the thermometer in it being at 55° degrees above 0. The pump was kept continually working for half an hour, and the ball of the thermometer often dipped into the æther; but no greater degree of exhauſtion or cold could be produced. The air being let into

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the receiver, the quickfilver in the thermometer rofe 10°, *viz.* to 38° below o.

EXPERIMENT LXI.

Frefh æther being put into the phial to what was remaining, the thermometer rofe to 30° above o: the pump was then worked again conftantly for half an hour; yet by the barometer-gage the degree of exhauftion was now not more than 16, and the degree of cold produced did not exceed the 11th degree below o, as appeared by the quickfilver in the thermometer. The air being let into the receiver, the remaining æther was examined, and there were found feveral pieces of ice at the bottom of the phial, fome of them as big as large peas, which, when the æther became nearly of the heat of 32° or freezing point of water, were intirely diffolved.

The air-pump with which thefe experiments were made exhaufted above 400 times before the æther was put under the receiver.

XXXIII. *On the Culture of Pine-apples. An extract of a Letter from* William Baſtard, *Eſq. of* Kitley *in* Devonſhire, *to* Samuel Muſgrave, *M. D. F. R. S. dated* Kitley, March 15, 1777. *Communicated to the Society by Dr.* Muſgrave.

Read June 19, 1777.

BEFORE I enter into the particulars of raiſing pine-apples in water, it will be neceſſary to tell you that my hot-houſe is covered with the beſt crown-glaſs, which I apprehend gives more heat than the common ſort of green glaſs generally uſed for hot-houſes. In the front part of the houſe, and indeed any where in the loweſt parts of it, the pine-apple plants will not thrive well in water. The way in which I treat them is as follows. I place a ſhelf near the higheſt part of the back wall, ſo that the pine-plants may ſtand without abſolutely touching the glaſs, but as near it as can be: on this ſhelf I place pans full of water, about ſeven or eight inches deep; and in theſe pans I put the pine-apple plants, growing in the ſame pots of earth as they are generally planted in to be plunged into the

 bark-

bark-bed in the common way; that is, I put the pot of earth, with the pine-plant in it, in the pan full of water, and as the water decreafes I conftantly fill up the pan. I place either plants in fruit, or young plants as foon as they are well rooted, in thefe pans of water, and find they thrive equally well: the fruit reared this way is always much larger, as well as better flavoured, than when ripened in the bark-bed. I have more than once put only the plants themfelves without any earth, I mean after they had roots, into thefe pans of water, with only water fufficient to keep the roots always covered, and found them flourifh beyond expectation. In my houfe, the fhelf I mention is fupported by irons from the top, and there is an intervening fpace of about ten inches between the back wall and the fhelf. A neighbour of mine has placed a leaden ciftern upon the top of the back flue (in which, as it is in contact with the flue, the water is always warm when there is fire in the houfe) and finds his fruit excellent and large. My fhelf does not touch the back flue, but is about a foot above it; and confequently the water is only warmed by the air in the houfe. Both thefe methods do well. The way I account for this fuccefs is, that the warm air always afcending to the part where this fhelf is placed, as being the higheft

part of the houſe, keeps it much hotter than in any other part. The temperature at that place is, I believe, ſeldom leſs than what is indicated by the 73d degree of FAHRENHEIT's thermometer, and when the Sun ſhines it is often at above 100°: the water the plants grow in ſeems to enable them to bear the greateſt heat, if ſufficient air be allowed; and I often ſee the roots of the plants growing out of the holes in the bottom of the pot of earth, and ſhooting vigorouſly in the water.

My hot-houſe (the dimenſions of which it may be proper to know) is ſixty feet long, and eleven feet wide the flues included; ſix feet high in the front, and eleven feet at the back on the inſide of the houſe. It is warmed by two fires. A leaden trough or ciſtern on the top of the back flue is preferable to my ſhelf, as in it the pine-plants grow much faſter in the winter, the water being always warmed by the flue: of this I have ſeen the great benefit theſe laſt two months in my neighbourhood. It is not foreign to this purpoſe to mention that, as a perſon was moving a large pine-plant from the hot-bed in my houſe laſt ſummer, which plant was juſt ſhowing fruit, by ſome accident he broke off the plant juſt above the earth in which it grew, and there was no root whatever left to it: by way of experiment I

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took the plant, and fixed it upright in a pan of water (without any earth whatever) on the ſhelf; it there ſoon threw out roots, and bore a pine-apple that weighed upwards of two pounds.

XXXIV. *Experiments and Obſervations made in* Britain, *in order to obtain a Rule for meaſuring Heights with the Barometer.* *By* Colonel William Roy, *F. R. S.*

Read June 12 and 19, and Nov. 6 and 13, 1777.

INTRODUCTION.

IN philoſophical inquiries of every kind, where any point is to be aſcertained by experiments, theſe cannot be repeated too often, nor varied too much, in order to obtain the truth: for even when the utmoſt precaution hath been uſed, and the greateſt pains have been taken, it rarely happens, that they agree ſo exactly, as to leave no room for doubt. Were it poſſible at all times, to have experiments made in circumſtances perfectly ſimilar, a conſiderable degree of conſiſtency might naturally be expected among the reſults, whereof the mean would determine the point in queſtion; but different men, making uſe of different inſtruments, have different modes of conducting their operations, each purſuing the tract that ſeems to him the moſt likely to inſure ſucceſs. Hence it is that a variety

of

of refults arife, and that things fometimes appear contradictory, or at leaft prefent themfelves under new forms, difficult at firft fight to be accounted for, and therefore apt to miflead, till by a farther inveftigation of the matter, the true caufes are difcovered. Even irregularities of this fort are worthy of being communicated, that others may know what hath happened before, and what, in like cafes, they may expect to meet with, in the courfe of their future inquiries. Improvements of every kind advance by flow degrees; and it is not until things have been viewed in every poffible light, that the errors, even of our own experiments, are difcovered, the points in queftion ultimately afcertained, and the branch of philofophy depending upon them, gradually brought nearer to perfection.

Ever fince the difcovery made by TORRICELLI, the barometer hath been applied, by different perfons, in different countries, to the meafurement of vertical heights, with more or lefs fuccefs, according to the more or lefs perfect ftate of the inftruments ufed, and the particular modes of calculation adopted, by the obfervers. But of all thofe who have hitherto employed themfelves in this way, none hath beftowed fo much time and pains, or fucceeded fo well, as Mr. DE LUC, of Geneva, F. R. S. In two quarto volumes, publifhed fome years fince, that gentleman

gentleman hath given us the hiſtory of the barometer and thermometer, with a very curious and elaborate detail of many years experiments, made by him, chiefly on the mountain Saleve. It would be totally ſuperfluous here to enter into any circumſtantial account of the method he makes uſe of, ſince that hath already been ſo fully illuſtrated by two Fellows of the Royal Society, who have at the ſame time given formulæ and tables, adapted to the meaſures of this country, (Phil. Tranſ. for 1774, vol. LXV. N° XX. and XXX.) that nothing farther can be deſired on that head.

It may nevertheleſs be neceſſary juſt to call to remembrance that the rule, deduced from the obſervations on Saleve, conſiſts of three parts. 1ſt, The equation for the expanſion of the quickſilver in the tube, from the effect of heat, whereby the heights of the columns, in the inferior and ſuperior barometers, are conſtantly reduced to what they would have been in the fixed temperature of $54^{\circ}\frac{1}{4}$ of FAHRENHEIT, independant of the preſſure they reſpectively ſuſtained. 2d, When the mean temperature of the column of air to be meaſured, is 69°.32, as indicated by thermometers expoſed to the Sun's rays at its extremities; then the difference of the common logarithms, of the equated heights of quickſilver in the two

two barometers, gives the altitude intercepted between them, in toifes and thoufandth parts, reckoning the three figures to the right hand decimals, and the others integers, the index being neglected. This temperature of 69°.32, when the logarithmic differences give the real height without any equation, is reduced to 39°.74, the new zero of Mr. DE LUC's fcale, when his formula is adapted to Englifh fathoms and thoufandth parts, inftead of French toifes. And laftly, when the mean temperature of the air is above or below 39°.74, an equation, amounting to $\frac{21}{10000}$ parts of the logarithmic height for each degree of difference, is, in the firft cafe to be added to, and in the laft fubtracted from, that refult, in order to obtain the real altitude.

In Mr. DE LUC's book, the experiments for afcertaining the expanfion of the quickfilver, are not given in detail; neither are the particular temperatures of the barometers fpecified. The winter feafon was however chofen for the purpofe; one being left in a cold room, and the other in a clofet, heated as high as could conveniently be fuffered. The operation having been repeated feveral times without any effential difference in the refults, this general conclufion is drawn, that between the temperatures of melting ice and boiling water, the expanfion of the

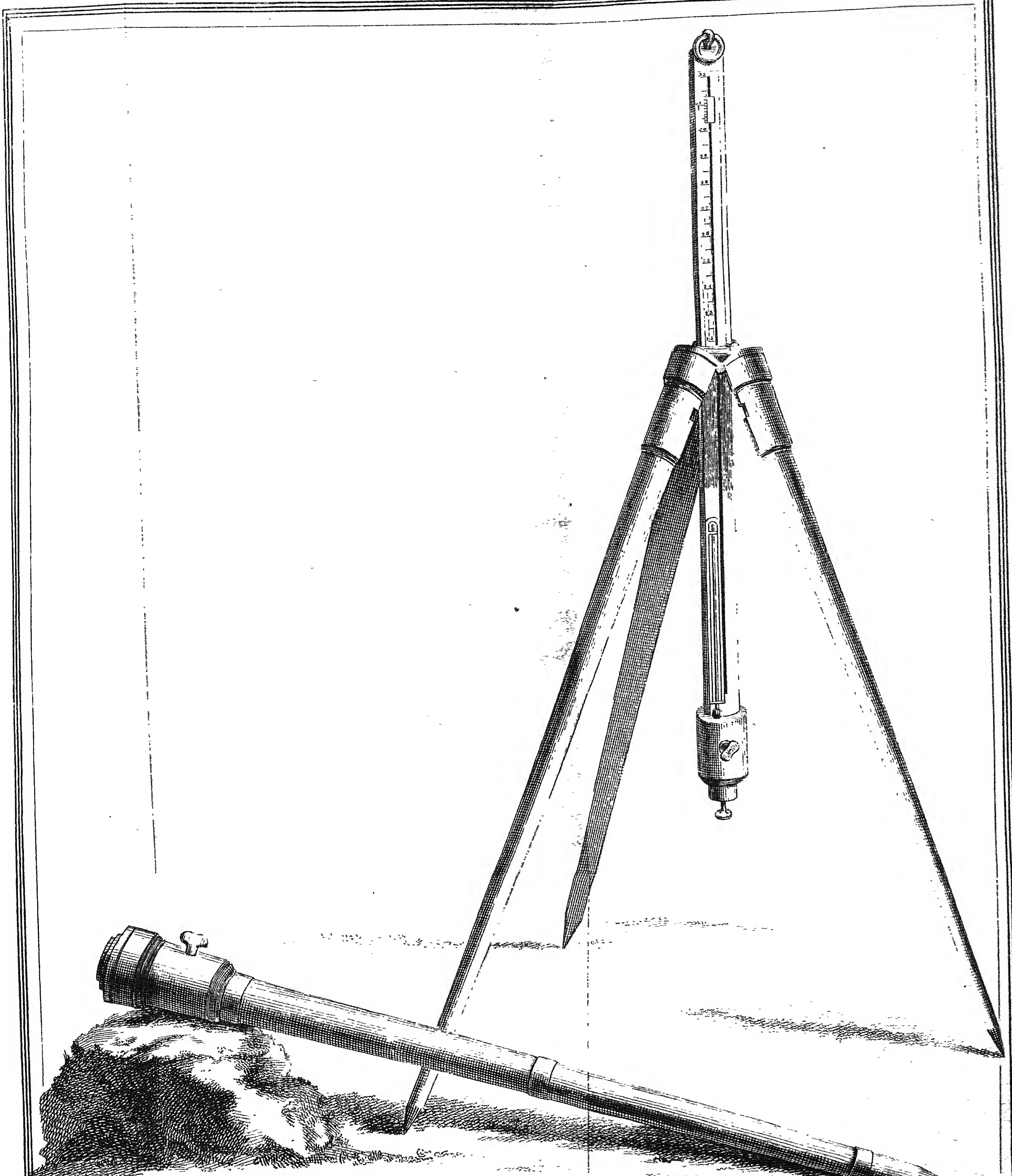
the quickfilver is exactly fix French lines, or .532875 decimal parts of an Englifh inch. But it is to be obferved, that the barometer ftood then at 28.77525; whereas, if it had ftood at 30 inches, it would have been .555556, becaufe the expanfion is in proportion to the length of the column. Farther, the interval between the freezing and boiling points in all thermometers, varies with the height of the barometer, or weight of the atmofphere; and it is the cuftom in England to make thermometers when the barometer ftands at 30 inches; that is to fay, 1.225 or 13.8 French lines, higher than when Mr. DE LUC's boiling point was fixed: and fince from his experiments it appears, that each line of additional height in the barometer, raifes the boiling point $\frac{1}{1134}$th part of the interval between that and freezing, it follows that $\frac{180}{1134} = 0.158 \times 13.8 = 2°.2$, will denote the number of degrees, that Mr. DE LUC's boiling point is lower than that of Englifh thermometers, which reduces it to 209.8 of FAHRENHEIT, and makes the interval between freezing and boiling only 177.8 degrees. Hence the expanfion .555556, formerly found, muft be increafed in the proportion of 177.8 to 180, which gives for the total .5624297 or .56243, on a difference of temperature of 180°. Thus the expanfion for each degree, fuppofing it

 to

to be arithmetical, or uniformly the ſame in all parts of the ſcale, will be .00312461[(a)].

Having now ſhewn the expanſion of quickſilver in the tubes of barometers reſulting from the Geneva obſervations, I ſhall next proceed to give ſome account of thoſe I made for that purpoſe. They derive their origin from my having very accidentally obſerved, that a ſmall degree of heat, and of ſhort duration, ſenſibly affected the length of the column in Mr. RAMSDEN's portable barometer, whereof a view is given in plate XVI. The principal parts of this inſtrument are a ſimple ſtraight tube, fixed into a wooden ciſtern, which, for the conveniency of carrying, is ſhut with an ivory ſcrew, and, that being removed, is open when in uſe. Fronting this aperture is diſtinctly ſeen, the coincidence of the gage-mark, with a line on the rod of an ivory float, ſwimming on the ſurface of the quickſilver, which is raiſed or depreſſed by a braſs ſcrew at the bottom of the ciſtern. From this, as a fixed point, the height of the column is readily meaſured on the ſcale attached to the frame, always to $\frac{1}{500}$

(a) This paper having lately been communicated to Mr. DE LUC, he hath informed me, that the difference of temperature in his experiments, amounted to about 31° of REAUMUR, or 72° of FAHRENHEIT, above freezing: wherefore, $.00312461 \times 72 = .225$ nearly, will denote the rate of expanſion from which he deduced that for 180°; and within theſe limits, it will hereafter be found to differ very little from the reſult of the preſent experiments.



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part of an inch, by means of a nonius moved with rack-work. A thermometer is placed near the ciftern, whofe ball heretofore, was ufually inclofed within the wood work, a defect that hath been fince remedied. The three-legged ftand, fupporting the inftrument when in ufe, ferves as a cafe for it, when inverted and carried from place to place. Two of thefe barometers, after the quick-filver in them hath been carefully boiled, being fuffered to remain long enough in the fame fituation, to acquire the fame temperature, ufually agree in height, or rarely differ from each other more than a few thoufandth parts of an inch, which were conftantly allowed for in calculating altitudes, as well as in eftimating the rate of expanfion, in the courfe of the following experiments.

SECTION I.

Experiments on the expanfion of quickfilver.

THE experiments made for this purpofe were numerous as well as various, and were therefore fubdivided into feveral claffes. To give a minute detail of them all, would be extremely tedious, and now wholly ufelefs, fince it was from thofe of the third clafs alone, that the

rate as well as maximum of expanſion was aſcertained: wherefore thoſe of the two preceding claſſes need only be mentioned in a general way.

The firſt ſet of the firſt claſs comprehended ſuch as were made with one barometer in a cold room, or in the open air, and the other in a room on the ſame level with the former, where there was conſtantly a fire, which was occaſionally increaſed, in order to augment the difference of temperature. When the heated barometer had remained ſeveral hours in an angle of the room, the difference of temperature of its quickſilver above that of the coldeſt, as indicated by their reſpective attached thermometers, rarely exceeded 10 or 12°, which, from a mean of many obſervations, gave an expanſion of .0333 decimals of an inch, for the 10° comprehended between 32 and 42° of FAHRENHEIT's thermometer. So far the reſult ariſing in this way, from ſmall differences of temperature, will be found to agree with the third claſs of experiments.

But when, in the ſecond ſet of this firſt claſs, the difference of temperature was augmented to 20 or 30°, by expoſing the barometer within doors to a greater heat, or placing the ſuperior one on the leads, whereby it received the direct and reflected rays of the Sun throughout the greateſt part of the day, while the other was kept in

in the cold area underneath, the rate of expanſion for the firſt 10° exceeded that formerly found nearly in the proportion of three to two, while that for the ſecond and third terms, of 10° each, diminiſhed progreſſively.

The chief, though not the only cauſe of this great difference, as will appear hereafter, aroſe from the poſition of the ball of the thermometer, originally incloſed within the wood-work of the frame, which prevented it from receiving the heat ſo readily as the quickſilver in the tube; at the ſame time that it retained it longer, and conſequently produced reſults in ſome degree fallacious.

Finding, from the firſt claſs of experiments, that much uncertainty remained with regard to the rate of expanſion of quickſilver affected by theſe ſmaller degrees of heat, and that it was utterly impoſſible, from them, to determine its maximum for the 180° between freezing and boiling; I reſolved to try, how much a column of 30 inches of quickſilver, carefully boiled in a tube, would lengthen, the ſame being placed with the open end upwards in a tin veſſel, occaſionally filled with pounded ice and water, and afterwards brought to boil, by means of a charcoal fire placed underneath? In this ſecond claſs, it was eaſy to ſee, that the expanſion of the tube containing the quickſilver, was neceſſarily to be taken into the account, and added to that apparently found by experi-

ment. This was of courſe to be done, either by ſuch differences as I could diſcern and meaſure, or by thoſe that had reſulted from the experience of others.

The nature of the apparatus, employed in this claſs of experiments, will be eaſily underſtood from plate XVII. where it is repreſented, as it was uſed in thoſe of the third claſs. In its firſt ſtate it was not quite ſo long, and a chafing-diſh with a charcoal fire, occupied the place of the ciſtern holding the quickſilver below. By means of a circular bit of tin, ſoldered edgeways in the center of the bottom, and an aperture in the middle of the lid, the tube was kept ſteadily in the axis of the veſſel. Other openings in the lid, ſerved for the admiſſion of the thermometer, and the application of a deal rod cloſe to the ſide of the tube, when its height was to be meaſured. The longitudinal expanſion of the glaſs was marked by a ſcratch thereon with a fine edged file at the top of the deal rod, when reſpectively at the temperatures of freezing and boiling. The apparent dilatation of the quickſilver was in like manner marked, by the coincidence of its ſurface with the lower edge of a braſs ring embracing the tube.

It having been found impoſſible to procure tubes whoſe bores were truly cylindrical, or of any uniform figure, the experiment was repeated, as often as poſſible,

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in both ends of the same tube, that the mean might be taken. But it frequently happened that the tube, which had undergone one or more experiments in one end, broke before any could be made with it in the other. In this case, the rate of expansion in the last end was taken from that given by such another tube, where it had succeeded in both. The mean of five results with the best tubes, taken in this way, gave .4901 for the apparent expansion of 30 inches of quicksilver, on 180° of FAHRENHEIT, between freezing and boiling, which being augmented by the apparent longitudinal dilatation of the glass $.0356 \times 3 = .1068$, the real expansion is .5969; exceeding Mr. DE LUC's by more than $\frac{3}{100}$ths of an inch. If, however, Mr. SMEATON's dilatation of glass, ($.025 \times 3 = .075$) be substituted, instead of that resulting from these experiments, the real expansion of 30 inches of quicksilver will be .5651, which does not exceed it quite $\frac{3}{1000}$ parts of an inch.

In this class of experiments, having attended as diligently as possible to all the circumstances, it seemed to me, that tubes with a small bore, and whose glass was thick, lengthened more than those, which had a larger bore and whose glass was thin: whence I was led to suppose, that solid glass rods would dilate more in proportion, and consequently, shew a still more perceptible difference.

difference. With the view of afcertaining this point, I procured four glafs rods near three feet long each, and of different diameters, the largeft being of the fize of the little finger, and the fmalleft about the thicknefs of a quill. One end of each, was fomewhat larger than the other, and was made perfectly fmooth, as that on which they were to reft when feverally meafured with the deal rod. They were then all placed in the tin veffel, in fuch a manner, as to admit pounded ice rammed very clofe around them, and the interftices to be filled with water. Having remained in that ftate a full half hour, they were feverally meafured with the deal rod, whofe length of $32\frac{1}{2}$ inches was fcratched on each with the fharp edge of the file. This being done, the ice thrown out, and the veffel carefully wafhed, all the rods were replaced in it, immerfed in water, which afterwards was brought to boil. The fire being kept up, and the ebullition rendered as violent as poffible for half an hour, the glafs rods were then feverally meafured, by applying them one after another to the deal rod, ftanding with them in the boiling water. The experiment was repeated three times, on as many different days, without its being poffible to difcern, that any of the glafs rods had dilated more than that of deal, from a difference of temperature of 180°. In all of them, the freezing mark feemed accurately to coincidewith the top of

of the deal rod; whereas the dilatation of the tubes, by the same degree of heat, was always very visible[b].

Finding from the second class of experiments, whereof the general result hath now been given, that glass rods seemed not to lengthen more than deal; and that tubes of different bores, and probably too of different sorts of glass, were susceptible of different degrees of extension, which rendered it impossible, by this means, to ascertain the longitudinal expansion of the quicksilver they contained; I thought it necessary to recur once more to the barometer, and to try whether it could not be so contrived as to act in water of different temperatures, from freezing to boiling. This led me to the experiments of the third class: and in order to comprehend them thoroughly, it seems necessary to point out some few alterations which the apparatus underwent.

The center of the bottom being pierced on purpose, a brass socket was prepared for it, wherein a hole was bored conically, to receive the ground-end of a barometer tube, of the ordinary length of $33\frac{1}{2}$ inches; the tube having been first ground in a separate piece of brass, and ulti-

(*b*) Since these experiments were made, the relative expansion of 18 inches of one of the tubes and one of the rods that had been formerly employed, was found to be, by Mr. CUMMING's pyrometer, nearly as 4 to 1, from a heat approaching to that of boiling oil.

mately in the ſocket itſelf, fitted it ſo exactly, as to ſuffer no water to paſs. The ſocket being inſerted into the aperture at the bottom of the veſſel, was firmly ſoldered to it for the reception of the tube, which was ſo ground as to reach a full inch and a half below the ſurface of the braſs. It could not deſcend farther, the ground parts in both being of the figure of the fruſtum of an inverted cone. From the view in the plate it will appear, that underneath the veſſel, a ſeparate ſtand was placed, in order to ſupport the iron ciſtern containing the quickſilver. The diameter of the ciſtern was ſuch, that its ſtand being occaſionally moved, ſo as to bring one ſide of it cloſe to the ground part of the tube, the other ſide projected beyond the bottom of the veſſel; and conſequently permitted the rod of a float, reſting on the ſurface of the quickſilver, to riſe freely and parallel to the axis of the tube. The rod was of deal, $\frac{1}{10}$th of an inch ſquare, carrying on its top a ſcale, whoſe zero lay in the lower ſurface of the float, and whereof the ſix uppermoſt inches, from 28 to 34, were divided into 20ths.

That the whole column of quickſilver might alternately be covered with the freezing mixture and water of different temperatures, and yet permit its ſurface to be ſeen, two eyes of plate glaſs were ſcrewed into ſockets, ſoldered for that purpoſe oppoſite to each other, near the

top

top of the vessel, which, in the first set of the third class of experiments, was little more than 29 inches high. The top of the tube passing through the aperture in the lid, one and a half or two inches of the vacuum generally rose above the vessel. That the expansion of the column might be measured as nearly as possible in that part of the tube fronting the center of the eyes, more or less quicksilver, according to the state of the atmosphere, was occasionally put into the cistern, to raise or depress the surface of the column to the proper height. A thin brass ring, whose lower parts were made to spring, embraced with sufficient force the upper part of the tube, permitting it at the same time to be moved freely with the hand. It carried along with it a nonius index, projecting as far as the center of the rod, and consequently applying itself to the divisions of the scale, which was kept in its proper position by passing through a slit fitted for it in an arm attached to the lid. The divisions on the nonius being the same with those of the barometer formerly described, the height of the quicksilver could always be read off to $\frac{1}{500}$th part of an inch.

The quicksilver having been carefully boiled, as on former occasions, in the tube; and that being filled completely, and held with its open end upwards; the tin vessel was inverted over it, and lowered gradually, till the

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ground end could be inserted into the socket with such a degree of force as to prevent it from being too easily removed. The finger being then applied closely to the open end of the tube, the whole apparatus was turned up, and placed over the cistern into which the quicksilver had previously been put, great care being taken not to remove the finger till the lower extremity of the tube was fairly immersed into the quicksilver; when that in the tube was permitted to descend into an equilibrium with the atmosphere. In the first experiment it was found that the water issued by the eyes, and running down the side of the vessel, fell into the cistern. In order to remedy this inconveniency, a circular piece of tin was soldered round the upper part of it, immediately below the eyes; and a flat spout, projecting from it, served as a gutter to throw off the water from the cistern, and from the lamps made use of to bring that in the vessel to boil. Six lamps, each with a double light, were suspended around the trunk of the vessel, to heat the water as equally as possible; though any irregularity of this kind was sufficiently guarded against, by constantly mixing it during the operation. Another lamp of the same kind stood under the cistern, whereby the quicksilver there was kept at the temperature of the water in the vessel, each having its proper thermometer: this last lamp was placed

and

and difplaced frequently, during the courfe of every experiment; for the heat was very expeditioufly communicated to the iron ciftern, and thence to the quickfilver it contained; and both were found to cool very faft, after the lamp was removed. Such was the ftate of the apparatus, when the firft fet of this third clafs of experiments was made. In thofe of the fecond fet, its height was farther augmented by tin foldered to the top, that a tube of the ordinary length might be wholly immerfed in boiling water. The third and laft alteration confifted in the occafional application of a detached tin cafe, equal in diameter to the upper part of the veffel, having a hole in its bottom to admit the top of a long tube to pafs. This cafe was fo contrived, that its bottom ftood two inches and a half higher than the lid of the veffel, thereby allowing room for the hand to move the index up or down. In this ftate the apparatus is reprefented in the view; and its various ufes will be beft underftood from the account of the experiments, which were fubdivided into four fets.

Thofe of the firft fet were made with tubes of a large bore, upwards of three-tenths of an inch in diameter, of the ordinary length, with a vacuum over the quickfilver of two inches and a half or three inches, part of which reached above the top of the veffel. The mean of three

experi-

experiments gave .5258, for the total dilatation of 30 inches of quickſilver, on 180° between freezing and boiling; that, anſwering to the firſt 20°, between 32° and 52°, was .0688; that, for the 20° in the middle of the ſcale, between 112° and 132°, was .058; and the rate for the laſt 20°, between 192° and 212°, was only .041. From this firſt ſet of the third claſs of experiments, it appeared evident, that the expanſion of 30 inches of quickſilver in the barometer, ſuffering a heat equal to 180° of FAHRENHEIT, inſtead of exceeding Mr. DE LUC's, as appeared to be the caſe from the reſults of the open tube, really fell ſhort of it: and inſtead of being arithmetical or uniformly the ſame, for equal changes of temperature, was actually progreſſive; the expanſion anſwering to the lower part of the ſcale, being greater than that correſponding to the middle; which again exceeded that for high temperatures. In theſe experiments, when the water had acquired a heat 20 or 30 degrees greater than that of the open air, a certain duſtineſs was perceived in the vacuum of the tube. At 100° of FAHRENHEIT, or thereabout, this appearance had ſo far increaſed, as to ſhew clearly, that it could proceed from no other cauſe than a vapour ariſing from the ſurface of the heated quickſilver, quite inviſible, till, by its condenſation in the cold part of the tube, it was formed into balls, every where adhering to

its

its sides and summit. These globules were very small near the surface of the water, augmenting gradually as they approached the top of the tube, where they were greatest: their bulk increased with the heat; and when the water was at or near boiling, they would sometimes unite, and descend by their own gravity, along the sides of the tube, into the general mass. Hence the progressive diminution of the rate of expansion of the column of quicksilver in the barometer, perceptible even in the first class of experiments, is easily accounted for by the resistance of the elastic vapour(c), acting against the top of the tube, which was here colder than the rest.

But in the application of the barometer to the measurement of heights, the whole instrument is of the same temperature; wherefore, in the second set of this third class of experiments, the tin vessel was heightened, that tubes of the ordinary length, placed in it, might be wholly immersed in boiling water. The mean of four experiments, which agreed very nearly among themselves, gave .5117 for the total expansion between freezing and boiling; for the 20°, between 112° and 132°.059; and for the last 20°, between 192° and 212°.046. In these

(c) Having mentioned the circumstances to Mr. RAMSDEN, it first occurred to him, that the resistance of the elastic vapour was the cause of the diminution in the rate of expansion.

experiments, the tube being wholly covered with boiling water, no condenſation of vapour took place in the vacuum; and therefore no particles of quick-ſilver were ſeen adhering to the upper part of the tube. When the water boiled, the reſiſtance of the vapour was greater than in the preceding ſet, and the total expanſion leſs. Theſe two reſults ſerve ſtrongly to confirm each other: it is, however, the laſt that furniſhes the data for conſtructing the table of equation depending upon the heat of the quickſilver in the barometer, of which table we ſhall give an account hereafter.

Finding, from the compariſon of theſe two ſets of experiments with each other, that the maximum and rate of expanſion ſeemed to vary with the length of the vacuum above the quickſilver, I was adviſed to try[(d)] what might be the reſult, when the vacuum was much longer than in the common barometer.

The third ſet of experiments of this claſs was therefore made with a tube ſomewhat narrower in the bore than the former, and whoſe vacuum was $14\frac{1}{2}$ inches in length, whereof $11\frac{1}{2}$ reached above the top of the veſſel. The mean of three obſervations gave .5443 for the total expanſion on 180°; that for the firſt 20° was .067; for

(*d*) Dr. BLAGDEN, who afterwards aſſiſted in ſome of the firſt experiments with the manometer, propoſed that with the long tube.

the

the 20° in the middle of the scale .058; and for the uppermost 20°.065: whence the mean rate for every 20°, is nearly .0605[(e)]. In this set, the condensation in the vacuum of the tube was particularly attended to: it began, as in those of the first set, immediately above the surface of the boiling water, which was always kept an inch or two above the top of the column: the lowermost globules were very small, increasing gradually till they got without the lid of the vessel, where they were the largest; thence they diminished uniformly upwards, and disappeared entirely three or four inches below the top of the tube. Though the rate for the middlemost 20°, in these last experiments, be below the mean, probably from some inaccuracy in observation; yet, being compared with the former sets, they still serve to corroborate each other: for in these with the long tube, the vacuum seems to have been either completely maintained, or nearly so; and we accordingly find the maximum of expansion increased, and its rate rendered nearly uniform, as will be farther confirmed from what follows.

(*e*) Mr. CAVENDISH, who assisted in the first part of the experiments with the open tube, informed me, that, in those made by his father Lord CHARLES, the difference between the expansion of quicksilver and glass, from 180° of heat, was .469. If to this we add Mr. SMEATON'S dilatation of glass, the total expansion of 30 inches of quicksilver will be .544, which agrees with the experiments in the long tube, and gives a rate of only .003022 for each degree.

 I have

I have already had occafion to mention that a detached tin cafe was fometimes applied above the veffel, in which ftate it is reprefented in the view. This method was thought of during the operations with the long tube, in order to try whether the vacuum was completely maintained by the temperature of the open air? For this purpofe the cafe was placed on the ftones of the yard, with a fmall tube inferted in it, to preferve an open paffage in the middle: it was then rammed full of a compofition of falt and ice; and afterwards fixed on the top of the long tube. The degree of cold thus applied round the greateft part of the vacuum, muft have been very great, probably near the zero of FAHRENHEIT; yet it produced no vifible alteration in the height of the column of quickfilver, which ftill remained in boiling water below, and fhould have rifen, if the vacuum had been formerly incompleat. As it would have occafioned much trouble to have lengthened the feveral parts of the apparatus fo as to have kept the long tube wholly in boiling water, the counterpart of this laft experiment was not made in the accurate manner it ought: neverthelefs, the tin cafe, being emptied of its cold compofition, was placed on the tube as before, and filled with boiling water; which, joining with the intermediate fteam arifing from that in the veffel below, muft have kept the whole nearly in the

fame

ſame temperature. The conſequence of this application was, that the column ſhortened about $\frac{2}{100}$ths of an inch; which ſeems to prove, that the quickſilver vapour now reached the ſummit of the tube, and, acting againſt it, overcame, by ſo much, the preſſure of the atmoſphere.

I ſhould now proceed to give ſome account of the fourth ſet of this laſt claſs of experiments, made on the condenſation of the quickſilver, by means of artificial cold, below the temperature of the air. Previouſly however to this, it may not be improper to take notice, in a more general way, of ſome others that were made on expanſion; as tending, with certain circumſtances yet to be mentioned, not only to confirm thoſe already deſcribed, but likewiſe to account for many irregularities that occur in operating with barometers.

In the courſe of the preceding experiments, from accidents of various kinds, it was often neceſſary to reboil the quickſilver; and in that operation, many tubes were broken. The frequent removal of the ſocket from the bottom of the veſſel, in order to get others ground for it, became at laſt very troubleſome; and made more caution neceſſary, in boiling ſuch as were ground, eſpecially in froſty weather, which happened to be the caſe in the laſt days of March, 1775: wherefore it was thought beſt in the interim to try, what might be the expanſion of a column

of quickſilver, carefully put into the tube, but not boiled therein?

With this view, the ſtandard barometer and apparatus were left out during the night of the 29th, that they might acquire the ſame temperature, which was found next morning to be $34^{\circ}\frac{1}{2}$; the unboiled quickſilver ſtanding $\frac{1}{100}$th of an inch higher than that which had been boiled. The lamps being applied to the veſſel, the lengthening of the unboiled column was perceived, on the whole, to be more irregular, and the progreſſive diminution quicker, than in former experiments; ſo as to give, for the maximum of expanſion, only .443 for 180°.

On the morning of the 31ſt, the unboiled column, which on the preceding day had been the higheſt, was lower than the other by near $\frac{2}{100}$ths of an inch, the temperature of both being $31^{\circ}\frac{1}{2}$. As the water acquired heat from the application of the lamps, the rate of expanſion diminiſhed; and, at boiling, was only .405 for 180°. The operation of the 30th ſeems to point out, in a manner ſufficiently concluſive, that the air contained in the unboiled quickſilver, rendered its ſpecific gravity leſs, than that which had been boiled even a great while before; ſince it required a longer column of the firſt, to counterbalance the weight of the atmoſphere. And though the vacua might poſſibly, at the beginning, have been equally compleat

compleat in both; yet they could not continue long ſo: for the air eſcaping gradually from the unboiled quickſilver, its elaſticity increaſing with the heat, and uniting with the quickſilver vapour, muſt have reſiſted the dilatation of the column, and rendered it leſs than on former occaſions; which actually appeared from experiment. This is farther confirmed by the obſervations of the ſubſequent day; for now the unboiled column was become the ſhorteſt, owing no doubt to more air having aſcended, and rendered the vacuum ſtill more incompleat. Thus, the cauſes of reſiſtance increaſing, the dilatation is leſſened in a ſuperior degree.

The other circumſtances to be mentioned, occurred on the 12th of April. After finiſhing one of the experiments of the ſecond claſs, and when the water had cooled to 192°, the veſſel, by accident, received a ſudden jolt, whereby the mouth of the tube muſt have been raiſed, for a moment, out of the quickſilver in the ciſtern. In a few minutes after this, intending to obſerve how far the column had ſhortened from the decreaſing heat, I was ſurprized to find, that the quickſilver had wholly diſappeared in the tube, and was ſunk ſo low as not to be ſeen by looking obliquely down at the eye of the veſſel. It was then certain that air, and probably a particle of moiſture along with it, had aſcended into the upper part of the tube, whereby

whereby the vacuum was deſtroyed in ſo remarkable a degree. Since this accident made it neceſſary to reboil the quickſilver, the water (then between 180° and 190°) was let out by the cock fixed for that purpoſe at the bottom of the veſſel; but before it could be intirely drawn off, the tube and its contents, had ſo ſenſibly felt the condenſing force of the ſurrounding atmoſphere, then about 48°, that the quickſilver had riſen again, and preſented itſelf oppoſite to the eye of the veſſel, ſomething lower indeed than where it formerly ſtood. On this diſcovery, and as ſoon as water could be boiled for the purpoſe, the veſſel was filled again, when the quickſilver ſubſided, as before, quite out of ſight; and on drawing off the water a ſecond time, it roſe anew, ſeemingly to its former height.

The appearance, which this accidental circumſtance produced, was ſuch, as naturally ſuggeſted that farther experiments might have been made, varied as much as poſſible from each other, by the admiſſion of different quantities of air, or of air and moiſture intermixed. But the nature of the veſſel rendering it impoſſible to ſee, and conſequently to meaſure, the motion of the quickſilver, occaſioned by the alternate expanſion and condenſation of the elaſtic vapour contained in the upper part of the tube, and which could not have been accom-

pliſhed

plished without many troublesome alterations in the apparatus, therefore nothing of the kind was attempted. From the circumstances just now mentioned, it will be readily conceived, how much care is necessary in operating with barometers for the measurement of heights, that the vacua be as nearly as possible compleat; and particularly, that no moisture get up into the tube. I now proceed to the fourth and last set of experiments.

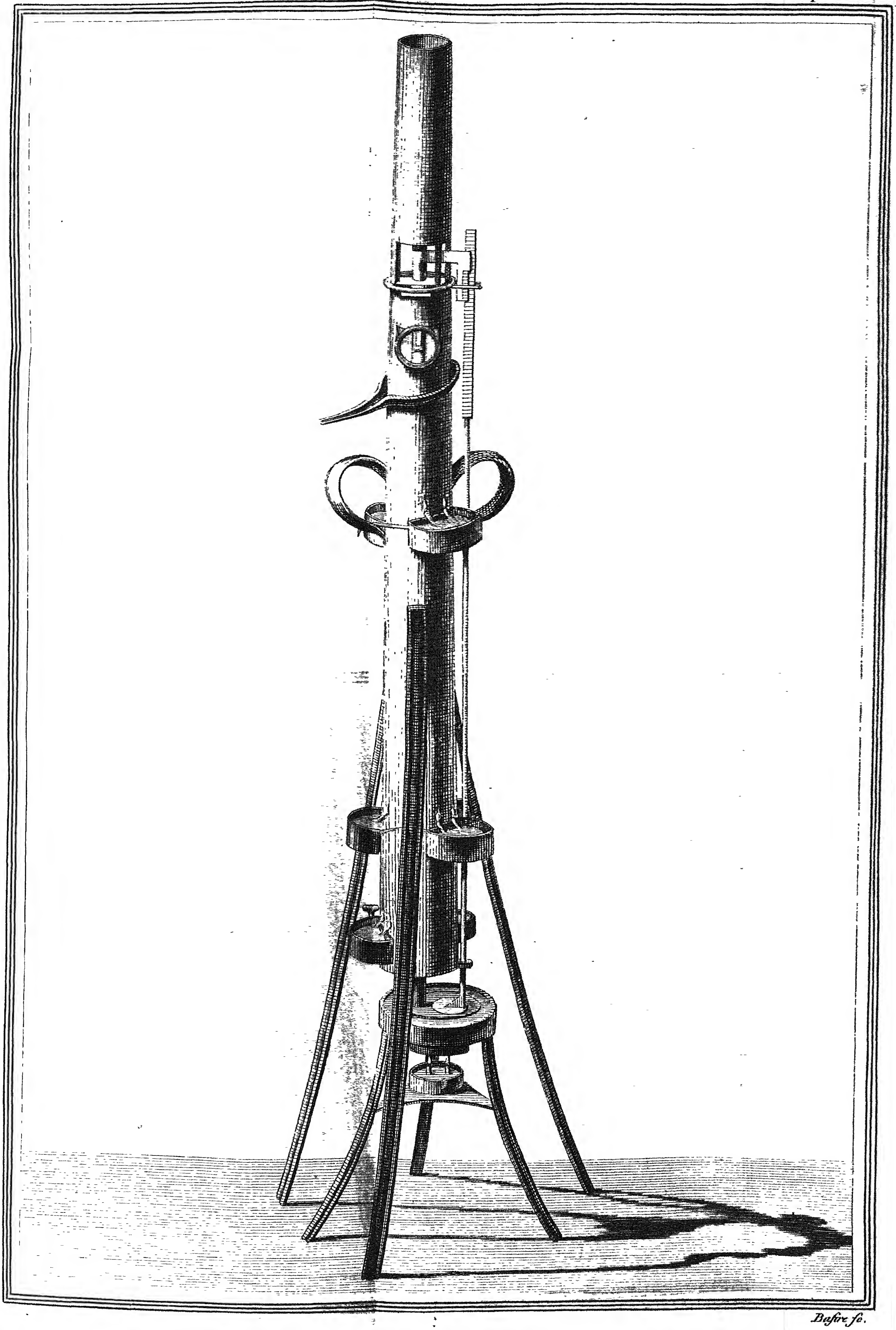
Having found, from the two first sets of this class, the rate of expansion of a column of quicksilver, in the tube of a barometer of the ordinary length, to be progressive and not arithmetical; and that its maximum, for the 180° comprehended between feeezing and boiling, was less than had been supposed; I thought it proper to try, by means of artificial cold, whether the condensation, for the 32° below freezing, followed nearly the same law?

For this purpose the tin vessel, containing the ground tube, was rammed quite full of pounded ice and salt, as well as the tin stand holding the iron cistern below. In this operation, twelve pounds of ice and four pounds of salt were employed, whereby the mean temperature of the mixture was reduced to +4° of FAHRENHEIT. But before the eyes of the vessel could be sufficiently freed from the composition, so as to permit the surface of the column to be distinctly seen and read off; it had risen to

+14°; the temperature of the air, and alfo of the ftandard barometer, being at the fame moment 49°½. The obferved condenfation, arifing from this difference of 35°½, was $\frac{12}{100}$ths of an inch; or .1189, when reduced for the height of the barometer, which then ftood at 30.296. Hence the condenfation for 32° is .1072, or .00335 for each degree. In this day's experiment, when the temperature of the mixture had rifen to 32°, that of the air and ftandard barometer was 52°¼; whence the reduced difference, for the 20° between 32° and 52°, was found to be .0664, anfwerable to former experiments.

The fame experiment was repeated two days after, with great care, the veffel being filled no higher than the furface of the quickfilver. The mean temperature of the mixture was now +4°, and that of the ftandard barometer 49°¼. The obferved condenfation, arifing from this difference of 45°¼, was $\frac{162}{1000}$; or .1594, when reduced for the height of the barometer, then ftanding at 30,416: hence the rate for 32° is .1127, or .003,522 for each degree. When the temperature of the mixture had rifen to 32°, that of the air was 51°: whence the augmented rate for the 20°, between 32° and 52°, was found to be .0662.

From the mean of thefe two experiments it appears, that the condenfation of a column of 30 inches of quickfilver



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quickſilver in the barometer, affected by the 32° of cold below freezing, is .1099: and that the expanſion from 20° of heat, between 32° and 52°, is .0663, a number agreeing perfectly well with former reſults. If the condenſation .1099 thus found, be added to the expanſion .5117 ariſing from the ſecond claſs of experiments, we ſhall have .6216 for the total difference of height of the columns of quickſilver in two barometers, ſuſtaining the ſame preſſure, but differing from each other in their temperatures 212° of FAHRENHEIT'S thermometer.

The ſeries of numbers expreſſed in the annexed table, agreeing in all eſſential reſpects with the expanſions found by experiment, will therefore ſhew that which correſponds to any intermediate temperature, for every 10° of the ſcale.

Rate of expanfion of a column of quickfilver in the tube of a barometer.

	Temperature.	Expanfions.	Differences.	2d Differences.
Expanfion above 32° of FAHRENHEIT; equation to be fubtracted from the height of the column of quickfilver of 30 inches.	212	.5117	.0229	
	202	.4888	.0236	
	192	.4652	.0243	
	182	.4409	.0250	.0007
	172	.4159	.0257	
	162	.3902	.0264	
	152	.3638	.0271	
	142	.3367	.0277	
	132	.3090	.0283	
	122	.2807	.0289	
	112	.2518	.0295	.0006
	102	.2223	.0301	
	92	.1922	.0307	
	82	.1615	.0313	
	72	.1302	.0318	
	62	.0984	.0323	
	52	.0661	.0328	
	42	.0333	.0333	
	32	.0000	.0338	.0005
Condenfation below 32° of FAHRENHEIT; equation to be added.	22	.0338	.0343	
	12	.0681	.0348	
	2	.1029	.0070	
	0	.1099		

Con-

Construction and application of the table of equation, for the expansion of the quicksilver in the tubes of barometers.

In the introduction to this paper there was occasion to remark, that in the application of the barometer to the measurement of heights, various modes of calculation had been adopted. The easiest and best method seems however to be, by means of the tables of common logarithms, which were first thought of by Mr. MARIOTTE, and afterwards applied by Dr. HALLEY, Mr. BOUGUER, Mr. DE LUC, and others. They have all proceeded on the supposition, that air is a truly homogeneous and elastic fluid, whose condensations being proportionable to the weights with which it is loaded, its dilatations are in the inverse of the weights; and in consequence of this law, that the heights of the atmosphere ascended, are in geometrical progression, while the corresponding successive descents of the quicksilver in the tube of the barometer, are in arithmetical progression.

Mr. DE LUC makes use of an arithmetical or uniform equation for the heat of the quicksilver in his barometers, whereby their relative heights are reduced to what they would have been in the fixed temperature of $54^{\circ}\frac{1}{4}$ of

of FAHRENHEIT. In the formulæ adapting his rule to Englifh meafures (Phil. Tranf. vol. LXVII. N° XX. and XXX.) hath been fhewn, that the eafieft and fimpleft method is, to make the difference of temperature of the two barometers the argument for the equation; and that it is fufficient to reduce either column to what would have been its height in the temperature of the other. But whatever may heretofore have been the method of ufing the equation for the heat of the quickfilver, while it was confidered as arithmetical; now that it hath been fhewn, from the preceding experiments, to be progreffive, there feems at leaft to be propriety in applying to each barometer the equation anfwering to its particular temperature. And though, for this purpofe, any fixed temperature might have been affumed at pleafure, as that to which both barometers were to be reduced; yet, the freezing point being fundamental in all thermometers, and that being likewife the zero of the fcale for the equation depending on the heat of the air, as will be fhewn hereafter, it hath been preferred to any other.

From the experiments it appears, that a column of quickfilver of the temperature of 32°, fuftained, by the weight of the atmofphere, to the height of 30 inches in the barometer, when gradually affected by different degrees of heat, fuffers a progreffive expanfion; and that, having

having acquired the heat of boiling water, it is lengthened $\frac{5117}{10000}$ parts of an inch: alſo, that the ſame column, ſuffering a condenſation by 32° of cold, extending to the zero of FAHRENHEIT, is ſhortened $\frac{1099}{10000}$ parts, the weight of the atmoſphere remaining in both caſes unaltered; but that, in the application of the barometer to the meaſurement of altitudes, ſince the preſſure and length of the column change with every alteration of vertical height, the equation, depending on the difference of temperature of the quickſilver, will neceſſarily augment or diminiſh by a proportionable part of the whole. Thus, if the weight of the atmoſphere ſhould at any time be ſo great as to ſuſtain 31 inches of quickſilver, the equation for difference of temperature will be juſt $\frac{1}{30}$th part more than that for 30 inches; at 25 inches it will be $\frac{5}{6}$ths; at 20 inches $\frac{2}{3}$ds; at 15 inches $\frac{1}{2}$; and at 10 inches only $\frac{1}{3}$d of that deduced from experiment.

It is upon theſe principles that the annexed table of equation hath been conſtructed, for differences of temperature extending to 102° of the thermometer, and for heights of the barometer from 15 to 31 inches; beyond which limits, it is not probable, that many barometrical obſervations will be made. The firſt or left-hand column, ſhews the height of the barometer for every half inch, from 31 to 25; thence for every inch downwards

to

to 20; the 15th inch being half of the obferved expanfion. The five next columns towards the right, comprehend the additive equation for condenfations, anfwering to 0°, 12°, and 22°, with their intermediate differences; thofe that are progreffive, as arifing from difference of temperature, being ranged horizontally; and thofe that are arithmetical, as depending on the height of the barometer, being placed vertically. The temperature of 32° requires no equation, and the thirteen columns from thence towards the right hand, contain the fubtractive equations for expanfion, correfponding to every 10° as far as 102°, with their progreffive and arithmetical differences ranged as before. By means of thefe differences, the equation for intermediate temperatures may readily be taken out by infpection. Hence is deduced the firft part of the rule for meafuring heights by the barometer. When the temperature of the quickfilver is below 32° of FAHRENHEIT, add the correfponding equation for condenfation to the obferved heights of the columns refpectively; when above 32°, fubtract the equation for expanfion from the obferved heights of the columns refpectively; with which equated heights of quickfilver, expreffed in 1000th parts of an inch, the tables of logarithms are to be entered.

Table

Table, ſhewing the equation to be applied to the obſerved height of quickſilver in the barometer, from 15 to 31 inches; and for differences of temperature extending to 102° of FAHRENHEIT: whereby the column is reduced to the height it would have ſtood at in the temperature of 32°.

Obſerved height of quickſil-ver in the Barom.	Condenſation below 32°; Equation to be added to the height of the quickſilver in the barometer.									Expanſion above 32°; Equation to be ſubtracted from the height of the quickſilver in the Barometer.																							
	0°		Diff.	12°		Diff.	22°		32°	42°		Diff.	52°		Diff.	62°		Diff.	72°		Diff.	82°		Diff.	92°		Diff.	102°	
31	.1136		.0432	.0704		.0355	.0349		When the temperature of the quickſilver in the barometer is 32° of FAHRENHEIT, no equation is neceſſary.	.0344		.0339	.0683		.0334	.1017		.0328	.1345		.0324	.1669		.0317	.1986		.0311	.2297	
30½	.1118		.0425	.0693		.0349	.0344			.0339		.0333	.0672		.0328	.1000		.0323	.1323		.0319	.1642		.0312	.1954		.0306	.2260	
30	.1099		.0418	.0681		.0343	.0338			0333		.0328	.0661		.0323	.0984		.0318	.1302		.0314	.1616		.0306	.1922		.0301	.2223	
29½	.1081		.0411	.0670		.0338	.0332			.0327		.0323	.0650		.0318	.0968		.0312	.1280		.0309	.1589		.0301	.1890		.0296	.2186	
29	.1063		.0405	.0658		.0331	.0327			.0322		.0317	.0639		.0312	.0951		.0307	.1258		.0304	.1562		.0296	.1858		.0291	.2149	
28½	.1045		.0398	.0647		.0326	.0321			.0316		.0312	.0628		.0307	.0935		.0302	.1237		.0298	.1535		.0291	.1826		.0286	.2112	
28	.1026	.00183	.0390	.0636	.00114	.0321	.0315	.000563		.0311	.000555	.0306	.0617	.001102	.0301	.0918	.00164	.0297	.1215	.00217	.0293	.1508	.002692	.0286	.1794	.003203	.0281	.2075	.003705
27½	.1008		.0384	.0624		.0314	.0310			.0305		.0301	.0606		.0296	.0902		.0291	.1193		.0288	.1481		.0281	.1762		.0276	.2038	
27	.0990		.0377	.0613		.0309	.0304			.0300		.0295	.0595		.0291	.0886		.0285	.1171		.0283	.1454		.0276	.1730		.0271	.2001	
26½	.0971		.0370	.0601		.0302	.0299			.0294		.0290	.0584		.0285	.0869		.0281	.1150		.0277	.1427		.0271	.1698		.0266	.1964	
26	.0953		.0363	.0590		.0297	.0293			.0289		.0284	.0573		.0280	.0853		.0275	.1128		.0272	.1400		.0266	.1666		.0261	.1927	
25½	.0935		.0356	.0579		.0292	.0287			.0283		.0279	.0562		.0275	.0837		.0269	.1106		.0267	.1373		.0261	.1634		.0256	.1890	
25	.0916		.0349	.0567		.0285	.0282			.0278		.0273	.0551		.0269	.0820		.0265	.1085		.0261	.1346		.0256	.1602		.0251	1853	
24	.0880		.0336	.0544		.0273	.0271			.0266		.0263	.0529		.0258	.0787		.0254	.1041		.0252	.1293		.0245	.1538		.0240	.1778	
23	.0843	.00366	.0321	.0522	.00228	.0263	.0259	.001126		.0255	.001110	.0252	.0507	.002204	.0248	.0755	.00328	.0243	.0998	.00434	.0241	.1239	.005384	.0235	.1474	.006406	.0230	.1704	.007410
22	.0807		.0408	.0399		.0251	.0248			.0244		.0241	.0485		.0237	.0722		.0232	.0954		.0231	.1185		.0225	.1410		.0220	.1630	
21	.0770		.0294	.0476		.0239	.0237			.0233		.0230	.0463		.0226	.0689		.0222	.0911		.0220	.1131		.0215	.1346		.0210	.1556	
20	.0733		.0280	.0453		.0228	.0225			.0221		.0220	.0441		.0215	.0656		.0211	.0867		.0210	.1077		.0204	.1281		.0201	.1482	
15	.0550	.0183	.0211	.0339	.0114	.0170	.0169	.00563		.0166	.00555	.0165	.0331	.01102	.0161	.0492	.0164	.0158	.0650	.0217	.0158	.0808	.02692	.0153	.0961	.03203	.0150	.1111	.03705

SECTION

SECTION II.

Experiments on the expansion of air in the Manometer.

WITH respect to order of time, the manometrical experiments were made subsequently to the chief part of the barometrical observations, from which alone an approximate rule had previously been deduced for the measurement of heights: nevertheless, in this paper it seemed to me best, that what related to the expansion of air in one instrument, should immediately succeed the expansion of quicksilver in the other.

The thermometer made use of in these experiments is above four feet long. Its scale extends from $-4°$ to $+224°$ of FAHRENHEIT, each degree being more than $\frac{2}{10}$ths of an inch: when the barometer stood at 30 inches, its boiling point was fixed in the tin vessel formerly described. Mr. RAMSDEN's thermometers generally rise in the same vessel $213°\frac{1}{2}$; and the long thermometer, being placed in the vessel he makes use of to fix his boiling points, rises only to 210°.

The manometers were of various lengths, from four to upwards of eight feet: they consisted of straight tubes,

5 whose

whofe bores were commonly from $\frac{1}{15}$th to $\frac{1}{25}$th of an inch in diameter. The capacity of the tube was carefully meafured, by making a column of quickfilver, about three or four inches in length, move along it from one end to the other. Thefe fpaces were feverally marked, with a fine edged file, on the tubes; and transferred from them to long flips of pafte-board, for the fubfequent conftruction of the fcales refpectively belonging to each. The bulb, attached to one end of the manometer at the glafs houfe, was of the form of a pear, whofe point being occafionally opened, dry or moift air could be readily admitted, and the bulb fealed again, without any fenfible alteration in its capacity.

The air was confined by means of a column of quickfilver, long or fhort, and with the bulb downwards or upwards, according to the nature of the propofed experiment. Here it muft be obferved that, from the adhefion of the quickfilver to the tube, the inftrument will not act truly, except it be in a vertical pofition; and even then, it is neceffary to give it a fmall degree of motion, to bring the quickfilver into its true place; where it will remain in equilibrio, between the exterior preffure of the atmofphere on one fide, and the interior elaftic force of the confined air on the other.

All the experiments were made when the barometer was at, or near, 30 inches. When the bulb was downwards, the height of the barometer at the time of obfervation, augmented, and when upwards, diminifhed by the number of inches of quickfilver in the tube of the manometer, expreffed the denfity of the confined air.

Pounded ice and water were ufed to fix a freezing point on the tube; and by means of falt and ice, the air was farther condenfed, generally four, and fometimes five or fix degrees below zero. The thermometer and manometer were then placed in the tin veffel, among water which was brought into violent ebullition; where having remained a fufficient time, and motion being given to the manometer, a boiling point was marked thereon. After this the fire was removed, and the gradual defcents of the piece of quickfilver, correfponding to every 20 degrees of change of temperature in the thermometer, were fucceffively marked on a deal rod applied to the manometer. It is to be obferved, that both inftruments, while in the water, were in circumftances perfectly fimilar; that is to fay, the ball and bulb were at the bottom of the veffel.

In order to be certain that no air had efcaped by the fide of the quickfilver during the operation, the manometer was frequently placed a fecond time in melting

 ice.

ice. If the barometer had not altered between the beginning and end of the experiment, the quickfilver always became ftationary at or near the firft mark. If any fudden change had taken place in the weight of the atmofphere during that interval, the fame was noted, and allowance made for it in afterwards proportioning the fpaces.

Long tubes, with bores truly cylindrical or of any uniform figure, are fcarcely ever met with. Such however as were ufed in thefe experiments, generally tapered in a pretty regular manner from one end to the other. When the bulb was downwards, and the tube narrowed that way, the column of quickfilver confining the air lengthened in the lower half of the fcale, and augmented the preffure above the mean. In the upper half, the column being fhortened, the preffure was diminifhed below the mean. In this cafe, the obferved fpaces both ways from the center, were diminifhed in the inverfe ratio of the heights of the barometer at each fpace, compared with its mean height. If the bore widened towards the bulb when downwards, the obferved fpaces, each way from the center, were augmented in the fame inverfe ratio; but in the experiments on air lefs denfe than the atmofphere, the bulb being upwards, the fame equation was applied with contrary figns: and if any

 extra-

extraordinary irregularity took place in the tube, the corresponding spaces were proportioned both ways from that point, whether high or low, that answered to the mean.

The observed and equated manometrical spaces being thus laid down on the paste-board containing the measures of the tube; the 212° of the thermometer, in exact proportion to the sections of the bore, were constructed along-side of them: hence the coincidences with each other were easily seen; and the number of thermometrical degrees answering to each manometrical space, readily transferred into a table prepared for the purpose.

I have already had occasion to remark that, from the operations of the barometer alone, an approximate rule, or mean equation, had been obtained for the measurement of heights; but as, among the results, irregularities were now and then met with, doubts naturally arose, whether the equation, instead of being considered as uniform, might not follow an increasing or diminishing progression? Without an infinite number of observations, in very different temperatures above and below the zero of the scale, this point could not possibly be determined by the barometer: wherefore the first and chief thing proposed to be discovered by the manometrical experiments was, whether common air, occasionally rendered

 more

more or lefs denfe, by the addition or fubtraction of weight, expanded equally with quickfilver, when affected with the fame degrees of heat? According to the ratio that took place between the expanfion of quickfilver and air, above and below zero, I intended to regulate the barometrical equation already found, without regarding the proportion of the bulb to the bore of the manometer; or in other words, without paying any attention to the actual expanfion of the air confined in that inftrument.

But after a great number of thefe firft experiments had been made, it was judged proper to compute the actual expanfion of 1000 equal parts of air in the manometer, from a heat of 212°; wherefore, in the laft, the accurate capacity of the bulb, with refpect to the bore, was determined; at the fame time that the original mode of comparing the thermometrical with the manometrical fpaces, was ftill adhered to.

It is eafy to conceive in experiments of this very delicate nature, part of which, namely thofe on air lefs denfe than the atmofphere, were extremely difficult and even laborious, that mathematical exactnefs was not to be looked for; and that, notwithftanding every poffible precaution was taken, irregularities would occur. Thefe, however, were not fo numerous as might have been

I expected,

expected, nor any way ſo great as to render the reſearch fruitleſs: for a few of that kind being thrown out of the total number, the mean of the others, which were very conſiſtent among themſelves, ſerved to prove beyond the poſſibility of doubt, that the expanſions of common air did not keep pace with the dilatations of quickſilver. The manometrical ſpace, anſwering to the 20° of the thermometer between 52° and 72°, was always found to be greater than any other 20° of the ſcale. Here it is to be underſtood, that I do not pretend to have aſcertained the exact point in that ſpace where the maximum falls: the mean correſponds to the 62d degree, and yet I am inclined to think that it is ſomewhat lower, perhaps it may be about the 57th: from this point, the condenſations of air downwards, and its expanſions upwards, follow a diminiſhing progreſſion, compared with the condenſations and dilatations of quickſilver. The manometrical are equal to the thermometrical ſpaces, in two points of the ſcale; namely, at or near the freezing temperature on one ſide, and between the 112° and 132d degrees of the ſcale on the other. At the zero and boiling point they are leſs than the thermometrical ſpaces. Whether this maximum of expanſion of air, compared with that of quickſilver, be owing to moiſture, or any

thing

thing elſe mixed with the former, which is brought into its greateſt degree of action, about the temperature of 57° of FAHRENHEIT, muſt be left to the inveſtigations of future experimenters: I only relate things as I found them after many repetitions, without being able to diſcover any material difference in the reſults, even when the air was rendered more or leſs denſe by an addition to, or ſubtraction from, the weight wherewith it was loaded. The thermometrical, compared with the manometrical ſpaces, will therefore appear as in the following table.

Spaces

Spaces of the quickfilver thermometer, FAHRENHEIT's fcale.	Spaces of the manometer, meafured in degrees of FAHRENHEIT.
212°	212°
20	17.6
192	194.4
20	18.2
172	176.2
20	18.8
152	157.4
20	19.4
132	138.0
20	20.0
112	118.0
20	20.8
92	97.2
20	21.6
72	75.6
20	22.6
52	53.0
20	21.6
32	31.4
20	20.0
12	11.4
0	0

Expe-

Experiments, for determining the actual expansion of common air, in the manometer, affected by the heat of 212°.

For this purpose it became necessary to ascertain, in every manometer, the exact proportion between the capacity of the tube and that of its bulb. This was done, by weighing the quicksilver that filled them respectively, in a balance that was sensible to a very small fraction of a grain. The contents of the bulb, and that part of the tube between it and zero, expressed in grains, was called the air in experiment. The apparent expansion of that air was measured, by the grains that filled the several sections of the tube between zero and the boiling point; the sum being the total expansion or increase of volume, from a heat of 212°. The apparent expansion, thus found, was again augmented for the dilatation of the tube, on the following principles.

In the first part of this paper I have shewn, that solid glass rods dilate much less than barometer tubes. The mean between Mr. SMEATON's and my experiments, gives $\frac{14}{1000}$ of an inch for the longitudinal extension of every foot of these tubes, by 212°. From the rate of going of a clock, for near a year, whose pendulum rod is solid glass, its dilatation seems to be one-third part of a steel rod,

or

or $\frac{58}{10000}$ on a foot, by 212°. Now, as the manometers resemble solid rods much more than they do barometer tubes, it is probable their dilatation, even allowing for the greater extension of the bulb, would not exceed $\frac{6}{1000}$ths of an inch on a foot, or $\frac{1}{1000}$th part on every two inches. In this ratio I have therefore augmented the apparent, to obtain the true, capacity of each manometer. The equation, amounting to about $\frac{1}{220}$th part of the whole, being less than the common error of such complicated observations, might in fact have been entirely omitted, without producing any material alteration in the results.

Having, in this manner, computed the total increased volume of any number of equal parts of air (according to the capacity of the bulb and tube in grains) and very often likewise the partial expansions for intermediate temperatures, expressed by the contents of the corresponding sections of the tube, I then found the ratio answering to 1000 equal parts, which, being divided by the degrees of difference of temperature, gave the mean rate for the whole scale, or the particular rate for any intermediate section of it.

The experiments, considered in this way, are distributed into four classes, whereof the results are comprehended in the four following tables. The first shews the expansion of air, whose density was much greater than that of the

common atmoſphere. The ſecond, which is divided into two ſets, contains thoſe on air that ſuſtained a preſſure leſs than the atmoſphere. In the third claſs, a very ſhort column of quickſilver being employed to confine the air, its denſity differed little from that we commonly breathe in: this claſs is likewiſe ſubdivided into two ſets, and it will hereafter be made uſe of to regulate the equation depending on the temperature of the air, in the application of the barometer. The fourth and laſt claſs of experiments, were made on air of the common denſity, artificially moiſtened by the admiſſion, ſometimes of ſteam, and at others of water, into the bulb; it is accordingly diſtinguiſhed into two ſets.

TABLE I. Reſults of experiments on the expanſion of air, whoſe mean denſity was equal to $2\frac{1}{2}$ atmoſpheres.

N°	Height of the barometer.	Inches of quickſilver confining the air.	Denſity in inches.	Total expanſion of 1000 equal parts of air by 212°.	Mean rate for each degree.
1	29.7	+72.	101.7	451.54	2.12991
2	29.7	+62.6	92.3	423.23	1.99637
3	29.62	+50.84	80.46	412.09	1.94382
4	29.66	+24.88	54.54	439.87	2.07486
5	29.66	+20.05	49.71	443.24	2.09075
		Mean,	75.74	434.00	2.04717

TABLE

TABLE II. Refults of experiments on the expanfion of air of the denfity of five-fixths of the common atmofphere; and of others on air that was extremely rare, being only preffed with about one-fifth of an atmofphere.

	N°	Height of the barom.	Inches of quick-filver.	Denfity in inches.	Total expanfion of 1000 equal parts of air by 212°.	Mean rate for each degree.	Expanfion for intermediate temperatures. From 0 to 32°.	32° to 52°.	52° to 72°	72° to 92°	92° to 132°	132° to 172°	172° to 212°
Firft fet.	1	29.85	—5.44	24.41	495.455	2.33705	—	—	Not obferved.		—	—	—
	2	29.76	—3.05	26.71	504.340	2.37896	2.27190	2.41666	2.64060	2.55200	2.46040	2.31850	2.20748
	3	29.79	—0.53	29.26	470.32	2.21849	1.90437	2.48150	2.63150	2.59650	2.15050	2.12000	2.10925
	4	30.09	—5.43	24.66	469.07	2.21259	2.32688	2.53450	2.66250	2.24800	2.25425	2.05325	1.83525
	5	29.93	—9.63	20.30	479.20	2.26038	2.14750	2.49500	2.59850	2.24700	2.25950	2.21375	2 11850
		Mean,		25.17	483.677	2.28140	2.16266	2.48191	2.63327	2.41087	2.28116	2.17637	2.06762

	N°		Difference of temperature.			Total expanfion.	Mean rate.
Firft fet.	6	Experiment in a heated room in Philpot lane, February 25, barometer 30.°03—4.82=25.21 the denfity of the air, —	on 113½° from	48½°	to 162°	244.604	2.15510
			51½	48½	100	126.311	2.45264
			62	100	162	118.293	1.90800
Second fet.	7	In Philpot lane; tube with a fmall bore; barometer 30.03—23.2=6.77 the denfity. The air had been heated red-hot in the bulb before it was fealed, — — —	on 113½ from	48½	to 162	138.75	1.22247
			51	48½	101½	71.93	1.41039
			60½	101½	162	66.82	1.10446
		The expanfion for 212°, at the mean rate, would be —	—	—	—	259.164	
	8	In Pulteney ftreet, February 28th; with the fame manometer that had been ufed in the fame experiment in Philpot lane, barometer 30.08—23.2=6.88 the denfity of the air, — —	on 212	—	—	330.487	1.55890
			32 above zero.			44.574	1.39294
			20 from	32	to 52	37.771	1.88855
			80	52	132	139.784	1.74730
			60	132	192	94.804	1.58007
			20	192	212	13.554	0.67770
	9	In Pulteney ftreet, April 19th; tube with a large bore, barometer 29.8—24.08=5.72 the denfity of the air, which had been heated red-hot in the bulb before it was fealed, — —	on 180 from	32	to 212	141.504	0.78613
			20	32	52	17.845	0 89225
			20	52	72	25.943	1.29715
			20	72	92	20.911	1.04550
			20	92	112	14.937	0.74685
			20	112	132	14.228	0.71140
			20	132	152	14.151	0.70755
			20	152	172	14.150	0.70750
			20	172	192	12.264	0.61320
			20	192	212	7.075	0.35375
		The Expanfion for 212° at the mean rate would be, —	—	—	—	166 660	
		Mean of the three means; denfity 6.46, expanfion for 212°,	—	—	—	252.104	1.18917

TABLE

TABLE III. Results of experiments on the expansion of air of the density of the common atmosphere.

	N°	Height of the barom.	Inches of quicksilver confining the air.	Density in inches.	Total expansion of 1000 equal parts of air by 212°.	Mean rate for each degree.
First set; common air.	1	29.95	+1.57	31.52	483.89	2.28250
	2	30.07	+0.70	30.77	482.10	2.27406
	3	29.48	+0.42	29.90	480.74	2.26764
	4	29 90	+0.83	30.73	485.86	2.29182
	5	29.96	+0.96	30.92	489.45	2.30870
	6	29.90	+0.65	30 55	476 04	2.24547
	7	29.95	+0 65	30.60	487.55	2.29976
Second set; common air heated red-hot	8	30.07	+0 53	30.60	482 80	2.27736
	9	29 48	+0 52	30.00	489.47	2.30871
			Mean	30.62	484.21	2.28401

The total expansion 484.21 being divided into parts proportionable to the manometrical spaces, measured in degrees of the quicksilver thermometer, as already given; we have the following expansions for intermediate temperatures, the rates for every 10° below 92° being found by interpolation.

Ther-

Thermometrical spaces.	Manometrical spaces.	Total Expansions for degrees above zero, 1000 parts.	Difference of expansions, 1000 parts.	Rate for each degree, 1000 parts.
212.°	212.°	484.210	40.199	2.00995
192.	194.4	444.011	41.559	2.07795
172.	176.2	402.452	42.949	2.14745
152.	157.4	359 503	44.310	2.21550
132.	138.	315.193	45.680	2.28400
112.	118.	269.513	47.507	2.37535
92.	97.2	222.006	24.211	2.42110
82.	86.6	197.795	25.124	2.51240
72.	75.6	172.671	25.581	2.55810
62.	64.4	147.090	26.037	2.60370
52.	53.	121.053	25.124	2.51240
42.	42.	95.929	24.211	2.42110
32.	31.4	71.718	23.297	2.32970
22.	21.2	48.421	22.383	2.23830
12.	11.4	26.038	26.038	2.16983
0.	——	——		

Hence 222.006 − 26.038 = 195.968 = 2.4496, or 2.45, is the mean rate of expansion for the 80° comprehended between 12° and 92° of FAHRENHEIT.

TABLE

TABLE IV. Refults of experiments on the expanfion of air, artificially moiftened, by the admiffion of fteam, and fometimes water, into the bulb of the manometer.

	N°	Height of the barom.	Inches of quickfiver confining the air.	Denfity in inches.	Total expanfion of 1000 equal parts of air by 212°.	Mean rate for each degree.	Expanfion for intermediate temperatures. from zero to 32°.	32° to 52°	52° to 72°	72° to 92°.	92° to 112°.	112° to 132°	132° to 152°.	152° to 172°	172° to 192°	192° to 212°
First fet: fteam admitted into the bulb before it was fealed.	1	30.16	+1.6	31.76	———	———	2.059375	2.60700	3.02650	3.38050	4.18300	6.48000	8.67750	11.93600	16.85050	———
	2	29.97	+2.2	32.17	1409.04	6.64642	2.20250	2.59250	2.90950	3.67650	5.16700	6.93300	10.17500	10.64200	16.57850	8.25400
	3	30.00	+2.2	32.20	1350.10	6.36840	2.26875	2.59100	3.04900	3.77550	4.36900	7.60500	8.94400	10.42950	11.92200	11.69000
	4	30.43	+1.92	32.35	———	———	2.20875	2.51450	2.74700	3.25500	3.73700	5.91350	9.18950	11.57550	25.88650	———
	5	30.2	+1.6	31.80	1999.71	9.43259	2.361875	2.51300	2.96400	3.84750	5.76100	7.19450	12.29850	16.69750	19.29500	25.23550
	6	30.32	+2.37	32.69	2576.16	12.15169	2.16250	2.55350	3.11600	3.72300	5.53600	7.83900	12.74100	16.74600	27.84350	45.25000
Second fet: a drop of cold water admitted into the bulb before it was fealed.	7	30.2	+1.3	31.50	1135.48	5.35604	2.22594	2.74450	2.90500	3.47750	5.41900	6.16650	7.98850	8.58950	10.93600	4.98600
	8	30.06	+3.2	33.26	———	———	2.54062	2.63350	2.80850	3.78700	4.60750	———	Tube broken.		———	———
	9	30.32	+1.6	31.92	1538.31	7.25618	2.02156	2.54250	3.22500	3.76500	5.41700	6.79250	9.14350	9.71100	13.75550	19.93270
Mean,				32.18	1668.13	7.86854	2.22799	2.58800	2.97228	3.63194	4.91072	6.86550	9.89494	12.04087	17.88344	19.22470
Mean of the fecond, third, and feventh,				31.96	1298.20	6.12362	2.23239	2.64267	2.95450	3.64317	4.98500	6.90150	9.03583	9.88700	13.14550	8.31000
Mean of the fifth, fixth, and ninth,				32.14	2038.06	9.61349	2.18198	2.53633	3.10167	3.77850	5.57133	7.27533	11.39500	14.38483	20.29800	30.13940

By N° 1. the total expanfion for 192° is 1208.72, whence the mean rate is 6.29542.
4. — — 192° 1367.05, — — 7.12005.
8. — — 112° 358.03, — — 3.19669.

From

From the experiments of the firſt claſs it appears, that 1000 equal parts of common air, loaded with two atmoſpheres and a half, being affected with a heat of 212°, expands 434 of thoſe parts; that is to ſay, in its dilated ſtate, it occupies a ſpace bearing, to that which it originally filled, the proportion of 1434 to 1000: hence the mean rate of expanſion of air of that extraordinary denſity is 2.04717 for each degree.

From the firſt ſet of the ſecond claſs of experiments it appears, that 1000 equal parts of air, preſſed only with $\frac{5}{6}$ths of an atmoſphere, and ſuffering a heat of 212°, expands nearly 484 of thoſe parts, whereof the mean rate for each degree is 2.28140. The maximum correſponds to that ſection of the ſcale between 52° and 72°; and the rate for the extremes is leſs than the mean.

But in the ſecond ſet of this claſs, when the confined air was rendered ſo extremely rare as to be preſſed with only one-fifth of an atmoſphere, in which caſe there was a neceſſity for heating it red-hot before it was poſſible to make the quickſilver hang in any tube of a moderate length, the expanſion of 1000 equal parts of air is, by the ſeventh and eighth experiments, diminiſhed to about two-thirds of the uſual quantity; and by the ninth, it is conſiderably leſs, amounting only to 141.5 for the 180°

 com-

comprehended between freezing and boiling, or 0.78613 for each degree. The maximum ſtill correſponds to the ſpace between 52° and 72°; and the minimum is conſtantly at the boiling point.

From theſe three laſt experiments it would ſeem, that the particles of air may be ſo far removed from each other, by the diminution of preſſure, as to loſe a very great part of their elaſtic force; ſince, in the ninth experiment, the heat of boiling water applied for an hour together, could only make it occupy a ſpace which, compared with what it filled at freezing, bears the proportion of 1141.5 to 1000.

From the third claſs of experiments it appears, that common air, preſſed with a ſingle atmoſphere, whether taken into the manometer in its natural ſtate, or heated red-hot therein, has the ſame expanſion with air of only five-ſixths of that denſity: for 1000 equal parts of this air expanded 484.21 from 212° of heat, whereof the mean rate is 2.28401 for each degree. By comparing this reſult with that of the firſt claſs, and again with that deduced from the ſecond ſet of the ſecond claſs, it would ſeem, that the elaſtic force of common air is greater than when its denſity is conſiderably augmented or diminiſhed by an addition to, or ſubtraction from, the weight

6 with

with which it is loaded[(f)]; for, in the firſt caſe, it bears the proportion of 484 to 434; and in the laſt, it is (from the

(*f*) This difference between the elaſtic force of common air, and that which is artificially rendered more or leſs denſe, by the addition or ſubtraction of weight, particularly the latter, is truly remarkable, and contradicts the experience of BOYLE, MARRIOTTE, &c. It could not ariſe from the adheſion of the quickſilver to the tube, though in the denſe experiments a column of 72 inches was once made uſe of; becauſe the conſtant motion given to the manometer before the ſpaces were marked, muſt either have prevented any irregularity whatever, or made the apparent expanſion ſometimes too great, and at others too little. But the rare experiments ſerve to put this matter out of doubt; for if the adheſion of the quickſilver to the tube had tended to leſſen the apparent expanſion of the air, beneath the truth in one caſe, it muſt have had a direct contrary effect in the other, and augmented it above the truth, which it evidently doth not.

Theſe experiments on the expanſion of air leſs denſe than the atmoſphere, were extremely difficult and troubleſome; and it was not till after ſeveral fruitleſs attempts that, with the aſſiſtance of Dr. LIND, an apparatus was prepared for making them with ſufficient accuracy. The veſſel employed for this purpoſe was made of the braſs tube of a large teleſcope, near four inches in diameter; it was divided into four pieces, which, when ſcrewed together, made a pot of ſix feet in height. This was mounted on a platform laid over the area rails, for the reception of the manometer, which was placed therein, with the bulb uppermoſt, the lower extremity of the tube paſſing through a ſocket at the bottom of the veſſel, and then through a collar compoſed of many thickneſſes of flannel. By means of a braſs plate and three long ſcrews, the collar was made to embrace the tube ſo cloſely, as to ſuffer very little water to paſs: ſuch as did iſſue, oozed off along the ſides of a paper funnel, bound round the end of the tube, without entering into the bore. In this poſition, it required ſome degree of force to puſh the manometer up, or draw it down, till the top of the quickſilver confining the air, juſt appeared without the collar, ſo as to admit the ſpaces to be meaſured, from a fixed point marked on the tube. The veſſel being filled with boiling water, was kept to that temperature by means of lamps ſuſpended

the mean of three experiments) as 484 to 252, when preſſed with only one-fifth of an atmoſphere.

The

pended around it. Two thermometers were made uſe of; the long one, whoſe ball ſtood at the bottom; and a ſhort one at the top, that deſcended no lower than juſt to be immerſed in the water. By ſome of the firſt of theſe experiments, the lamps not being placed directly at the bottom, water was perceived to be a very bad conductor of heat; for it would boil violently at the top, and the ſhort thermometer there would mark 212°, while the long one would only mark 185° or 190° at the bottom. By ſlow degrees the heat would nevertheleſs deſcend, and in the ſpace of fifteen or twenty minutes the whole column would become of the ſame uniform temperature. But when the apparatus was adapted for experiments on air denſer than the atmoſphere, in which caſe a plate of tin was ſoldered over the hole at the bottom, that it might be placed on a ſtrong fire, the heat was then greateſt below, and the long thermometer would mark upwards of 215°, while the ſhort one ſtood at 209° or 210°. By deſiſting from blowing the fire, or removing a part of it, the particles of water ſuffering the greateſt heat would aſcend, mix with the reſt, and for ſome little time make the whole column of an uniform temperature. But the fire being totally removed, the top of the column in cooling was always hotteſt; wherefore, in all theſe experiments, whether on denſe or rare air, great care was taken to mix the water thoroughly.

From Mr. DE LUC's book it appears, that M. AMONTONS found the effect of heat on the air confined in his thermometer, which ſeems to have been the ſame ſort of inſtrument with the manometer, proportionable to the weight with which it was loaded. By this he could not mean that, being of a double denſity, it had twice the expanſion. I apprehend it muſt here be underſtood, that the ſpaces the air occupied, were inverſely as the weights. That being preſſed with a double weight, it only filled half the ſpace; or with half the weight, a double ſpace. This is no doubt nearly, though not accurately, the law that it follows. From theſe experiments it appears, that there is little difference in the actual expanſion or elaſtic force of air, preſſed with an atmoſphere + or — one-third part: yet, when it is rendered extremely rare, its elaſticity is wonderfully diminiſhed. There ſeems likewiſe to be a viſible diminution in its expanſion, when loaded with two atmoſpheres and a half. Some of the tubes that I uſed were near nine

feet

The total expansion 484.21 resulting from the third class of experiments, which are very consistent among themselves, being divided into parts proportionable to the manometrical spaces, as measured in degrees of the quicksilver thermometer, we have the expansions for intermediate temperatures, expressed at the bottom of the third table, where, it is to be observed, the rates for every 10° below 92° were found by interpolation.

Now as barometrical observations will probably never be made in a temperature higher than 92° in the shade, nor in one lower than 12°, if we subtract 26.038, the expansion answering to 12°, from 222.006, that which corresponds to 92°, we shall have 195.968 for the 80 intermediate degrees; or 2.45 for the mean rate on each. This equation, compared with Mr. DE LUC's, bears the proportion of 245 to 210, which is a difference of $\frac{35}{100000}$ on every degree, or one-seventh part of the whole: and though this rate will be found hereafter to

feet long. Had it been possible to have managed them of double or triple that length, so as to have admitted the air to be pressed with a column of 18 or 20 feet of quicksilver, I am persuaded the diminution in the expansion of air of that extraordinary density would have been much more perceptible.

Mr. AMONTONS found, that the condensation of air in his thermometers kept pace with that of spirit of wine, which we are told follows a decreasing progression with respect to quicksilver: wherefore his experiments agree with these, in making the condensation of air below 57° follow a decreasing progression, when compared with that of quicksilver.

 exceed

exceed that deduced from the operations of the barometer in extreme temperatures; yet they agree exceedingly well with each other for the mean heat of the air, when the barometer will come moſt frequently into uſe.

The fourth claſs of experiments are all that now remain to be mentioned. The bare inſpection of TABLE IV. will ſhew, how greatly ſuperior the elaſtic force of moiſt is to that of dry air. It is true indeed, that two kinds of irregularities preſent themſelves among the reſults: firſt, with regard to the total expanſion for 212°; and ſecondly, as to the greateſt exertion of the elaſtic force, which ſometimes ſeems to have taken place before the air has acquired the heat of boiling water. The firſt is eaſily accounted for: it muſt have ariſen from different proportions of moiſture being admitted into the ſame quantity of air, which there was no poſſibility of aſcertaining, the bulbs and their apertures being of very different dimenſions. With regard to the ſecond irregularity, I am rather inclined to think that it may have proceeded from error of obſervation, it being difficult to determine the accurate temperature near boiling; eſpecially when any part of the air roſe above the top of the veſſel, which was ſometimes the caſe, notwithſtanding its extraordinary height. Be that as it may, a very uniform encreaſing progreſſion will be perceived to take place, from

from the zero of FAHRENHEIT, as far as 152° or 172°; and even to the boiling point, in thofe which I efteem the beft experiments. By adhering to the mean refult it will appear that air, however moift, having that moifture condenfed or feparated from it by cold, its expanfion differs not fenfibly from that of dry air. Thus the rate for 32° below freezing 2.22799, is nearly the fame as in dry air; but no fooner doth the moifture begin to diffolve and mix with the air, by the addition of 20° of heat, than the difference is perceptible: for inftead of 2.46675, the rate for 20° above 32° in dry air, we have 2.588 for that which is moift. In the next ftep of 20°, the rate for dry air is 2.5809; whereas that for moift is 2.97. In this manner the progreffion goes on continually encreafing, fo as to give 7.86854 for the mean rate on each degree of the 212°, which is near three times and a half the expanfion of dry air. And laftly, the rate for the 20° between 192° and 212°, is twice and one-half the mean rate, and about nine times that which correfponds to the zero of the fcale: but if the comparifon is drawn from the mean of the fifth, fixth, and ninth experiments, as being probably neareft the truth, the total expanfion of moift, will be more than four times that of dry air; and the rate for the temperature at boiling,

 will

will be nearly fifteen times that which correſponds to the zero of FAHRENHEIT.

I am aware it will be alledged, that the proportion of moiſture admitted into the manometer in theſe experiments, is greater than what can ever take place in nature; and therefore, in order to be able to judge of the degrees of expanſion the medium ſuffers in its more or leſs denſe, and more or leſs moiſt ſtates, that not only air near the ſurface of the earth, but likewiſe that found at the top of ſome very high mountain, ſhould have been made uſe of. I grant all this: but on the other hand it muſt be remembered, that theſe experiments are very recently finiſhed; that a good hygrometer (if ſuch can ever be obtained) a great deal of leiſure time, and the vicinity of high mountains, were all neceſſary for the carrying of ſuch a ſcheme into execution.

It is for theſe reaſons, and in hopes that other people will, ſooner or later, inveſtigate this matter ſtill farther, not only by experiments made on the expanſion of air, taken at different heights above the level of the ſea in middle latitudes, but likewiſe on that appertaining to the humid and dry regions of the atmoſphere towards the equator and poles, that I have been induced to haſten the communication of this paper. In the mean time having proved, beyond the poſſibility of doubt, that a wonderful

difference doth exiſt between the elaſtic force of dry and moiſt air, I may be allowed hereafter to reaſon by analogy, on the probable effects this will produce, in meaſuring heights with the barometer; leaving it to others, much better qualified, to conſider how far it will affect aſtronomical refractions. In the following ſection I ſhall therefore give an account of the barometrical obſervations made in Britain, and compare them with ſome others made in diſtant countries.

SECTION III.

An account of the barometrical obſervations made in Britain, *wherein they are compared with ſome others of the ſame kind made in diſtant countries.*

THE revival of the inquiries into that curious and uſeful branch of philoſophy, whereby vertical heights are determined to a great degree of exactneſs, by the preſſure of the atmoſphere alone, we owe to Mr. DE LUC; who hath undoubtedly removed many of the difficulties that formerly occurred in the application of the barometer, and thereby encouraged others to attempt to overcome, ſome part at leaſt, of ſuch as remain.

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If the rule deduced from the obſervations on Saleve had been abſolutely free from exceptions, and if there had not been particular points in the theory concerning which the ingenious author himſelf ſeems to have entertained doubts, it would probably have been univerſally adopted, without undergoing any very ſcrupulous inveſtigation; but the obſervations made at Sun-riſing on Saleve, gave reſults that were defective, or leſs than the real height. In certain caſes, the equation for high temperatures, remote from the zero of the ſcale, appeared to follow a diminiſhing, and in others an increaſing progreſſion. Hence aroſe ſome cauſes of uncertainty, with reſpect to the ſpecific gravities of quickſilver and air, and the relative expanſion of one compared with the other; eſpecially when the atmoſphere happened to have more or leſs moiſture diſſolved in it. It was doubtleſs from conſiderations of this ſort, that Mr. DE LUC, in his book, ſo ſtrongly recommends the making of numerous ſets of obſervations, in different countries; that, by the united labours of all, this intereſting part of natural philoſophy, might be brought ſtill nearer to perfection.

With this general object in view, I am now to give an account of the principal barometrical obſervations that have been made in Britain, on heights determined geometrically with great care. Theſe heights are claſſed in the

the following liſt in ſix ſets, according to the diſtricts of the country wherein they are ſituated, and nearly in the order of time in which the obſervations were made.

		Height in feet.
N° 1. Heights in and near London.	St. Paul's Church-yard *(g)*, North-ſide, and iron gallery over the dome, — — —	281
	Top of Paul's-ſtairs, and the ſaid gallery, —	324
	Top of Scotland-yard wharf, and the dining-room of the Spaniard on Hampſtead-heath, —	422
	Great Pulteney-ſtreet, and the ſaid dining-room, —	352
	Pagoda in Kew gardens, — — —	$116\frac{1}{2}$
	Gun wharf in Woolwich Warren, and uppermoſt ſtory of Shooter's-hill inn *(h)*, — — —	444

(g) Mr. BANKS, aſſiſted by other gentlemen, meaſured very accurately with a line the height of the ball of St. Paul's above the floor of the church, which was found to agree, exceedingly near, with that taken from the engraved ſection of the building. The diſtance of the ball from the dining-room of the Spaniard, was found by a baſe meaſured on Hampſtead-heath; and their relative heights by the angle of depreſſion of the ball, taken with the aſtronomical quadrant from the ſaid dining-room. The heights of Paul's-ſtairs and Scotland-yard wharf, with reſpect to each other, were found by meaſuring from them ſeverally to the ſurface of high water in the Thames. And the relative heights of the church-yard and floor of the church with reſpect to the ſtairs, and of Pulteney-ſtreet with regard to the wharf, were obtained by levelling to them reſpectively.

(h) The height of Shooter's-hill inn, above Woolwich, was found by a baſe meaſured in the meadows from the Warren eaſtward. Lord MULGRAVE, Mr. BANKS, and Dr. SOLANDER, aſſiſted in the geometrical operations; as did Dr. BLAGDEN, Meſſ. DE LUC and LLOYD, in the barometrical obſervations.

		Feet.
N° 2. near Tay-bridge in Perthshire.	Station at the East-gate of Castle Menzie's gardens near the village of Weem, and top of Weem Craig,	700¼
	The said station, and top of Bolfracks Cairn, —	1076½
	The said station, and top of Dull Craig, —	1244½
	The said station, and top of Knock Farle, —	1364½
	The said station, and that at the rivulet of Glenmore, below the South observatory on Schihallien, —	1279¼
	The said station, and South observatory, — —	2098
	The said station, and Western summit of Schihallien,	3281
	Station at the rivulet of Glenmore, and the South observatory, — — — —	818¾
N° 3. near Lanark.	Level of the Clyde at Lanark-bridge, and station in the garden at Lanark, — —	362½
	Ditto level, and top of Stonebyre-hill, — —	654
	Robinhood's well, before Carmichael-house, and top of Tinto, four feet below the summit of the Cairn,	1642½
	Ditto well, and West end of Carmichael-hill, —	451½
N° 4. near Edinburgh.	Leith pier-head, and top of the Calton-hill, —	344
	Leith pier, and summit of Arthur's Seat, — —	803
	Leith pier, and Kirk-yetton Cairn, on the East-end of the Pentland hills, — — —	1544
	Calton hill, and ditto Cairn, — — —	1200
	Level of Hawk-hill study, and top of Arthur Seat,	702½
	Hawk-hill observatory, and bottom of the little rock on Arthur Seat, 7¼ feet below the summit, —	68[illegible]
	Hawk-hill garden-door, and ditto little rock, —	730¼
N° 5. near Linhouse.	Linhouse, and East Cairn-hill, 5 feet below the summit,	1176½
	Ditto, 18 feet below the top, — —	1165½
	Linhouse, and West Cairn-hill, 11 feet below the top,	1178½
	Ditto, and Corstown-hill, 4 feet below the top, —	386½
	Corstown-hill, and West Cairn-hill, — —	792
	Ditto, and East Cairn-hill, — — —	776½
N° 6. near Carnarvon in North Wales.	Carnarvon Quay, and Snowdon Peak, — —	3555
	Ditto, and summit of Moel Eilio, — —	2371

To enter into a minute detail of the geometrical operations, whereby the whole of these vertical heights

were

were determined, would be extremely tedious and uninteresting. That some idea may however be formed of the degree of accuracy with which they were ascertained, it will be sufficient to observe, that the requisite angles were taken with an astronomical quadrant of a foot radius, made by Mr. SISSON, and curiously adapted for the measurement of horizontal or base angles; which, as well as those of the vertical kind, might always be determined thereby to within ten seconds of the truth. The bases were measured with care; and, in order to ascertain the distances, the three angles of each triangle were, as often as possible, actually observed with the quadrant. That the variation of the line of collimation of the instrument, which was found to alter in carrying, might occasion no error, one or more of the angles of elevation, at each station, were taken on the arc of excess, as well as on the quadrantal arc. In all cases, the usual (i) allowances were made for curvature and refraction: and for the correction of the last, sometimes the angles of de-

(i) If the square of the distance be divided by the diameter of the earth, the quotient will give the curvature of the globe on that distance, or the excess of the apparent above the true level: and, by Mr. MASKELYNE's rule, the square of the distance being divided by the diameter of the earth, augmented by one-fourth part, we have the allowance for curvature and refraction; which last is supposed to raise the object, by an angle equal to that of a great circle subtended by one-tenth part of the distance.

preſſion as well as of elevation were taken. When time would permit, the geometrical operations were repeated at the firſt ſtations; or the angles of elevation were obſerved from ſome new point connected with the firſt, and whoſe relative height, with reſpect to the others, was known. Small altitudes were occaſionally determined by levelling from one ſtation to the other.

To prove that the vertical heights, aſſigned to the places in the preceding liſt, are exceedingly near the truth, I need only mention the following inſtances, by way of confirmation. In 1771, with the aſſiſtance of Dr. LIND and his friend Mr. HOY, I meaſured a baſe from the obſervatory of Hawk-hill weſtward, whereby the height of the ſummit of Arthur's Seat, above the teleſcope of the Hawk-hill quadrant, in its horizontal poſition, was found to be 685.66 feet. In 1775, theſe gentlemen levelled, three ſeveral times, from the ſummit downwards to the ſaid teleſcope; and found the vertical diſtance to be, by the firſt operation, 686.47 feet; by the ſecond, 684.43; and by the third, 685.25. This laſt, which, from the great care that was uſed, they conſidered as the beſt, differs only three-tenths of a foot from the geometrical reſult. They afterwards continued the operation of levelling from Hawk-hill to the pier of Leith, and having repeated it twice, with a difference of only two inches between

between the results, they found the mean descent to be 117.38 feet: hence Arthur's Seat is above Leith pier, by the mode of levelling, 802.66; and by the mixed method 803 feet.

In 1774, when the astronomer-royal was carrying on the Society's experiments for ascertaining the attraction of Schihallien, I found, from my own geometrical operations, depending on a base measured in the plain near Taybridge, the Western summit of the mountain to be 1183 feet above the South observatory.

Of this height, the effect of curvature and refraction amounted to 28.86 feet, on the distance of Bolfracks Cairn from the observatory; and to 38 feet, on the distance of the said Cairn from the summit of Schihallien. The result of these operations I communicated to Mr. MASKELYNE, before his trigonometrical operations were begun. From the data which he hath since been so obliging as to furnish me with, depending on the base in Glenmore at the bottom of Schihallien, and the angles of elevation taken from the Southern extremity of that base, the Western summit of the mountain is 1186.6 feet above the South observatory. But if the triangle that served to connect the station of the barometer in that valley with the others, and the angles of elevation taken from the same station are made use of, the dif-

 ference

ference of height will be 1183.33 feet. Laftly, Mr. MASKELYNE'S refult, from the triangles on the North-fide of the mountain, makes it 1180. The mean of thefe three 1183.31 feet, is the height of the Weftern fummit of the mountain above the South obfervatory, which only exceeds my height by one-third of a foot. Here it is to be obferved, that from the vicinity of thefe triangles to the mountain, and the fhortnefs of the fides, the greateft curvature amounted only to 16 or 17 inches, which confequently made the effect of refraction next to nothing. This near agreement between the refults feems therefore to prove, that the mode of computation for curvature and refraction, made ufe of in the Taybridge obfervations, is juft.

By the firft angles of elevation, taken from the ftation of the barometer in Glenmore, the Weftern fummit of Schihallien is 2001.88 feet above it; from which, if we deduct 1183.33, there remains 815.55 for the height of the South obfervatory above the faid ftation: but if the laft angles of elevation at the ftation of the barometer are made ufe of, the height between it and the obfervatory comes out 818.97, whereof the mean is 818.76 feet. Though thefe inftances are of themfelves fufficient to prove, that the geometrical heights may be fafely depended upon; yet, as an example of the method that

was

was always made ufe of, I fhall annex to this paper, a plan of the triangles and detail of the operations for obtaining the height of Snowdon; becaufe that mountain, at the fame time that it is the higheft I have meafured, is, from its fituation, more likely to be vifited, and to have experiments repeated upon it, than the remote hills of the North. I now proceed to give fome account of the barometrical obfervations.

The heights in and near London being fo very inconfiderable, it was eafily forefeen, that nothing conclufive could be drawn from obfervations made on them alone. It was, however, natural enough to try, even on thefe, whether the rule we had been furnifhed with would anfwer? A fmall height of 41 feet 4 inches, which, without inconveniency, could be recurred to at all times of the day, and all feafons of the year, was the firft that was made ufe of. St. Paul's, Hampftead, Kew pagoda, and Shooter's-hill, were the next. The mean refults of many obfervations on the three firft, and of feveral on Shooter's-hill, were found to be defective. In general the coldeft obfervations, made in the morning and evening, when the temperatures at the two ftations differed leaft from each other, anfwered beft. In the hotteft part of the day, when that difference was the greateft, the refults were moft defective.

Some

Some months ſpent in Scotland in the ſummer of 1774, afforded opportunities of making barometrical obſervations on hills of various heights, from three or four hundred, to upwards of three thouſand feet, as hath been exhibited in the preceding liſt. That ſeaſon was remarkably cold and wet; wherefore, in theſe obſervations, the mean temperature of the air in the ſhade was commonly about 55°. The hotteſt never exceeded 63° in the plain; and the coldeſt, namely thoſe on the higheſt mountains, were generally from 43° to 48°.

From the defect found in the reſults of theſe obſervations, which, with reſpect to temperature, correſpond to the mean and hotteſt of thoſe made at Sun-riſing on Saleve, and without any exception whatever, I could eaſily diſcover, either that a much greater equation than what the rule directed, muſt be applied for each degree of heat above the zero of the ſcale; or, that the zero itſelf would fall conſiderably lower than 39°74, where Mr. DE LUC's formula, adapted to Engliſh meaſures, hath fixed it. This firſt ſtep towards a correction of the rule, naturally pointed out the ſecond thing to be aimed at, namely, the obtaining of a ſufficient number of cold obſervations, near the zero, and as far as poſſible below it, that the equation might diſappear entirely, and even come to be applied with the contrary ſign. Of this kind the winter

I ſeaſons

seasons of 1774 and 1775 afforded a few on the small heights in and near the metropolis; but the best I have been furnished with are those which Dr. LIND, assisted by Mr. HOY, was so obliging as to make on Arthur's Seat near Edinburgh; and those which Captain CALDERWOOD has since favoured me with on the Cairn-hills, being a part of the Pentland range to the South-west of that city.

By comparing these sets of observations together, it appeared from all of them, that when the air was at or near the freezing temperature, the logarithmic differences gave the real height, in English fathoms and thousandth parts, without any equation; and when considerably below that point, the equation was to be subtracted, or applied with the sign − instead of +. It was farther perceived, that the same general conclusion might be drawn from the coldest, not only of the Sun-rising, but even of the ordinary observations on Saleve, some reduction of the temperature being in certain cases made, on account of the exposure of the thermometer to the Sun's rays: hence I was led to suppose, that the morning observations, instead of being made exceptions from the rule, were those, which, it might be presumed, would form the best basis for deducing the equation depending on the heat of the air; because the mean temperature of the column was then found to differ least from that of its

extre-

extremities; whereas in the hotteſt time of the day, that difference was generally the greateſt.

Having been enabled, by means of the cold obſervations, to form ſome judgement whereabout the zero of the ſcale would fall, below which the equation was negative, and above it affirmative; it followed of courſe, that the next principal thing to be ſought for, was the maximum of equation, or that correſponding to the higheſt temperatures the climate of our iſland would afford. It was partly with the view of obtaining theſe that I went, in July 1775, to Snowdon in North Wales. On this expedition Captain CALDERWOOD was ſo obliging as to accompany me, and lend me his aſſiſtance in the operations for determining the geometrical height of that remarkable mountain. At that particular period, the weather proved unfavourable for obtaining hot barometrical obſervations; but, in other reſpects, they were very ſatisfactory, as being in general conſiſtent among themſelves, and agreeing ſufficiently near with thoſe of the preceding year in Scotland; at the ſame time that they were made on a height, as formerly mentioned, greater than any other hitherto meaſured, with equal care, in Britain. During the ſummer of 1776, Dr. LIND obtained ſome more hot obſervations on Arthur's Seat; and in the beginning of the following winter, Captain CALDERWOOD

5 made

made others of the cold kind, on the Cairn-hills in his neighbourhood. From the combination of the whole of theſe obſervations taken together, and a compariſon of them with Mr. DE LUC's, as far as they are ſimilar, I mean to ſhew the agreement or otherwiſe, between the equation for the heat of the air, as deduced from the barometer and manometer; but ſince the Britiſh obſervations, in certain caſes, differ conſiderably in their circumſtances from thoſe on Saleve, it is neceſſary, in the firſt place, to point out wherein this difference chiefly conſiſts.

In the obſervations in Britain, the barometers and detached thermometers have been, almoſt [illegible] conſtantly, placed in the open air in the ſhade, and ſuffered to remain there generally half an hour, and ſometimes a great deal longer, before the correſponding obſervations were begun, that the quickſilver might have time to take the temperatures of the ſituations reſpectively. They were then obſerved four times, uſually at intervals of ten minutes, the mean of the four being that which is calculated, and called a ſingle obſervation. If the time did not admit of ſo long an interval, the ſame number of obſervations were taken at diſtances of five minutes from each other. In either caſe, the extremes never differed above a few thouſandth parts of an inch from the mean, ſo as to

render the computations of them feparately wholly unneceffary.

Except in very fmall heights, and chiefly in London, where it was impoffible to fcreen the upper barometer fo effectually from the Sun during the time of obfervation as that below, which generally ftood in the fhade of fome building, the temperature of the quickfilver in the fuperior (k) hath been colder than that in the inferior barometer. The difference was commonly found to be two or three degrees; fometimes it would amount to fix or feven; rarely, in heights that were confiderable, to nine or ten; and in one inftance only to thirteen, where the vertical diftance of the inftruments was great.

Whether in the plain or on the tops of the higheft mountains, the detached thermometers, indicating the temperature of the air, have generally ftood fomething

(k) I have fometimes found, particularly in frofty weather, that a thermometer placed on the pavement of the North-fide of St. Paul's Church-yard, clofe to the wall of the building, would ftand two degrees lower than that which was expofed on the North-fide of the iron gallery over the dome. The firft, no doubt, felt the cold produced by the evaporation from the ftones, while that above might be affected by the afcending fmoke. But the moft remarkable inftance of this kind occurs in one of Dr. LIND's obfervations, on the breaking up of the hard froft January 31, 1776: at Hawk-hill, at $10^h\ 45'$ A.M. the temperature of the open air was 14°, while that at the fummit of Arthur's Seat was 20°. The froft that remained in the ground kept the air extremely cold below, though it had already felt the effects of the thaw at the top of the mountain.

lower

lower than thofe attached to their refpective barometers, until they had remained a confiderable time in the fame fituation, equally fhaded from the Sun, when they always agreed: whence it followed, that in thefe obfervations, the mean temperature of the air, and equation depending upon it, might always have been determined very near the truth, from the temperature of the quickfilver in the tubes, as fhewn by the attached thermometers, without ever confulting the detached ones. Let us now fee what were the circumftances attending the obfervations on Saleve.

Mr. DE LUC's lowermoft barometer ftood in the ground-ftory of a houfe near Geneva, where it remained unaltered during the whole of his experiments; while the detached thermometer, indicating the temperature of the air, was expofed on a fmall eminence, at a little diftance, directly to the Sun's rays: hence we find that, in the obfervations of high temperatures, the bottom of the column of air is often 12° or 15°, and in one cafe 18°, hotter than the quickfilver in the tube. And even in the loweft temperatures, the bottom of the column of air is generally hotter than the quickfilver within doors, contrary to common experience in this country: for in England, in winter, the exterior air in the fhade is always colder than the interior air. This circumftance gives

reafon to apprehend, that the thermometer fuffered not only direct but reflected heat.

The fuperior barometer was fhaded with a parafol from the Sun, while its correfponding detached thermometer was expofed to his rays: wherefore, in the obfervations of high temperatures, the top of the column of air is ufually four or five degrees hotter than the quickfilver in the barometer ftanding in the fame air; and the mean heat of the column often exceeds very confiderably the mean heat of the quickfilver in the tubes.

In many of the coldeft of Mr. DE LUC'S obfervations, as well as in thofe of mean temperatures of about 50° or 55°, the fuperior barometer is often the hotteft of the two, even when the furrounding atmofphere at the top is colder than at the bottom. This circumftance is eafily accounted for: wood is known to be a bad conductor of heat, to receive it flowly, and retain it long: that barometer, which was moved about from place to place upon the mountain, with a very fhort interval between the obfervations (as is fufficiently evident from the great number of ftations it paffed through in a limited time) muft have acquired and retained heat fuperior to that of the atmofphere, and communicated it to the tube with which it was in contact. Some difference would no doubt arife from this caufe, if the temperatures of the quickfilver

quickſilver in the tube and attached thermometer did not keep exactly pace with each other.

The laſt point to be mentioned is ſtill more remarkable than the reſt; it is briefly this: in the obſervations at Sun-riſing on Saleve, though the ſuperior quickſilver is the coldeſt; yet the top of the column of air is commonly five or ſix, and ſometimes eight or nine degrees, warmer than the bottom.

Having thus ſhewn the ſteps that were taken, for obtaining the coldeſt and hotteſt barometrical obſervations that the climate of this iſland would afford, the mode of obſerving, and wherein the circumſtances attending them differed from thoſe on Saleve, I ſhall now point out the general reſult. In order to avoid repetitions as much as poſſible, it is neceſſary, once for all, to remark, that the computations of the Britiſh obſervations, by the rule hereafter to be given, are ſubdivided into their reſpective claſſes. Each table contains 15 columns, which their titles ſufficiently explain, that the principles from which the rule was deduced, the reſult and error, might all appear in one view. The laſt column towards the right-hand ſhews the ratio of the weight of quickſilver to air, the columns of the firſt in the barometers being ſeverally reduced to the mean temperature of the laſt.

 By

By comparing the tables it will be found, that the obſervations for extreme temperatures belong to the Edinburgh claſs of obſervations (N° 4.) it being thought beſt, in this caſe, to omit the few hot ones obtained on the inconſiderable heights near London: the mean of the coldeſt, anſwering to the temperature of 21°.75, make the logarithmic exceſs $\frac{29}{1000}$; and the mean of the hotteſt, correſponding to the temperature of 69°.6, give a defect of $\frac{81}{1000}$. Now the ſum of the two equations $\frac{110}{1000}$, being divided by the difference of temperature 47°.85, we have nearly 2.3 for the mean rate of the equation on each degree, which is leſs than that reſulting from the operations of the manometer. Again, from the mean of the very beſt obſervations, as being made on the greateſt heights, when the temperature of the air is 52°, it appears, that the defect is from $\frac{49 \text{ to } 50}{1000}$, or 2.5 for each degree nearly, which agrees perfectly well with the manometrical expanſion. In this caſe, the ratio of the weight of quickſilver to air is as 11377 to 1; greater very conſiderably than 11232 to 1, aſſigned to them by Mr. DE LUC, when the temperature is 69°.32, anſwering to the zero of his ſcale, without any allowance for the diminution of preſſure on his columns, which ſhould have rendered air ſtill comparatively lighter. From the Britiſh obſervations, made on the moſt conſiderable heights,

it appears, that when the temperature of the air is 28°.2, the ratio of its weight, with reſpect to that of quickſilver, is as 1 to 10552: hence the increaſe of the weight of air, on every degree of difference of temperature between 28°.2 and 52°.5, amounts to 34.4; and hence we have 52°.5−4°.2=48°.3 for the temperature of the air in Britain, when its weight would be $\frac{1}{11232}$ of that of quickſilver; and conſequently agree with Mr. DE LUC's, though the heat would differ from his 21°. It will no doubt be remarked, that the equation for the air, reſulting from the operations of the barometer, falls ſhort of that given by the manometer. Part of the difference, I apprehend, may ariſe from the ſmall number of barometrical obſervations obtained in extreme temperatures. I ſhall, nevertheleſs, adduce reaſons hereafter for ſuppoſing, that it really ſhould diminiſh, becauſe of the drier and leſs elaſtic ſtate of the ſuperior air, compared with that taken into the manometer at the earth's ſurface. In the mean time, ſince both inſtruments agree in the equation for 52°, which is a heat that the barometer will very frequently be uſed in, it ſeems beſt to adhere to the mean manometrical reſult 2,45, in fixing the zero of the ſcale, which is obtained in the following manner.

Divide the exceſs or defect, expreſſed in 1000th parts of the logarithmic reſult, by 2.45, the mean expanſion of

air:

air for each degree of the thermometer; the quotient will give the number of degrees, in the firſt caſe, to be added to, and in the laſt ſubtracted from, the temperature of the air in the obſervation; the ſum or difference anſwers to the zero of the ſcale, or that temperature when the logarithmic reſult gives the real height in Engliſh fathoms and 1000th parts.

According to this mode of computation, we have, from the aggregate of the ſeveral claſſes of Britiſh obſervations, the place of the zero as follows:

						Zero.
By the 1ſt claſs of obſervations in and near London, between the temperatures of — —	25°.5	and	71°.2	at	32°.2
2d, near Taybridge, — —	46.1	—	62.9	—	31.1
3d, near Lanark, — —	44.	—	62.	—	32.8
4th, near Edinburgh, — —	17.	—	70.7	—	31.3
5th, near Linhouſe, — —	26.1	—	46.5	—	29.0
6th, near Carnarvon, — —	40.1	—	62.3	—	32.9
Mean place of the zero at — —					31.7

The number 31°.7 differing ſo very little from 32°, we may hereafter conſider that remarkable point of FAHRENHEIT's thermometer, as the zero of the ſcale depending on the temperature of the air; and hence is deduced the ſecond part, of the rule for meaſuring heights with the barometer. When the mean temperature of the column of air to be meaſured is at 32° of FAHRENHEIT, the

difference

difference of the common logarithms of the equated heights of quickfilver in the inferior and fuperior barometers, expreffed in 1000th parts of an inch, gives the real height in fathoms and 1000th parts, the three figures towards the right-hand being decimals, and the reft integers; which, being multiplied by fix, gives the refult in feet.

Let us next confider, in a general way, how far this will correfpond, or otherwife, with Mr. DE LUC's obfervations in extreme temperatures.

I have already had occafion to remark, that when the temperature of the air was at 69°.32, as indicated by thermometers expofed to the Sun's rays, Mr. DE LUC found that the differences of the common logarithms of the heights of the barometers at the two ftations, gave the altitude between them, in French toifes and 1000th parts: in which cafe the fpecific gravity of quickfilver to air was as 11232 to 1. When his formula is adapted to Englifh meafures, the zero of the fcale neceffarily defcends to 39.74, where the Englifh fathom bears the fame proportion to the modulus of the common logarithms, as, in the former cafe, the French toife did to that modulus, the equation for the intermediate temperature being now applied with the contrary fign. As it hath been fhewn, that the Britifh obfervations differ in their

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circumſtances from thoſe on Saleve, and require a greater equation, it is unneceſſary to enter into any minute compariſon of the two ſets: nevertheleſs, that ſome idea may be formed of the cauſe, of part at leaſt, of the difference that takes place between them, I have collected into one view, the computations of ſuch as were made in extreme temperatures; namely, the coldeſt of thoſe at Sun-riſing (though the whole of that claſs were conſidered as exceptions from the rule); the coldeſt and hotteſt of the ordinary obſervations; alſo thoſe on the Dole[(l)], at Genoa, and at Turin, whereby the heights of the lake of Geneva and of Turin, above the ſea at Genoa, were obtained. In the table it will be obſerved, that there is a column for the reduced temperature of the air, on account of the expoſition of the thermometer to the Sun's

(l) Having recomputed the whole of Mr. DE LUC's obſervations on Saleve, and claſſed them according to the months in which they were made, I intended, at one time, to have given a general table, comprehending the mean reſults of all of them: however, this is now become unneceſſary, ſince a very reſpectable and ingenious member of this Society hath had opportunities of making many curious and intereſting obſervations on thoſe very heights, which cannot fail of being perfectly ſatisfactory; and who, at my requeſt, was ſo obliging as to determine the height of the Dole geometrically. On this mountain Mr. DE LUC had made barometrical obſervations, whoſe reſults differing conſiderably from the altitude, 4182 feet above the lake of Geneva, as taken by Mr. FATIO DE DUILLIER, made me ſuſpect there was an error. In fact it appears, from the laſt meaſurement, that the ſummit of the Dole is 4293 feet above the ſurface of the lake, which gives for the vertical diſtance of Mr. DE LUC's barometer 4210 feet.

rays: I apprehend that I have not exceeded, but rather fallen ſhort, in the reduction, to what would have been indicated by thermometers in the ſhade, perfectly free from direct and reflected heat, and with ſufficient time allowed between the obſervations. Be this as it may, it is of no importance, as no other concluſion is drawn from theſe obſervations, than that of ſhewing what, in my apprehenſion, might probably have been the caſe, if another mode had been adopted.

From the table it appears, that when the temperature of the air is at 29°.5, the logarithmic exceſs is $\frac{9}{1000}$; and at 75°.5 reduced temperature, the defect is $\frac{96}{1000}$. The ſum of the two equations $\frac{105}{1000}$ being divided by the difference of temperature 46°, we have, as in the Britiſh obſervations, nearly 2.3 for each degree, which is greater than that applied by Mr. DE LUC's rule, in the proportion of 23 to 21. That too ſmall an equation hath been made uſe of in theſe hotteſt obſervations, ſuppoſing the original zero and temperature to remain, is ſufficiently evident: for $\frac{96}{1000}$ being divided by 42° the difference of temperature, we have, as before, 2.3 very nearly for the equation anſwering to each degree.

Farther, if we conſider the ratio of the weight of quickſilver to air, actually reſulting from the obſervations themſelves, the ſame kind of error (for I cannot ſee

 it

it in any other light) ſtill exiſts. Thus, in the coldeſt of the morning, as well as in the ordinary obſervations, when the temperature is at or near freezing, the mean ratio of the weight of quickſilver to air, is about 10850 to 1. When the obſerved and reduced temperatures are reſpectively 41° and 35°, the ratio between them is that of 11.295 to 1, anſwering nearly to what hath been aſſigned to them when the heat is 69°.32. Again, in the hotteſt obſervations of the 14th and 15th of July 1759, and 20th of July 1760, on the higheſt, and conſequently the beſt ſtations, when the obſerved and reduced temperatures are reſpectively 81°.7 and 75°.6, quickſilver is to air as 12650 to 1. Now if we reduce this number 12650 by a proportionable part, for the degrees of difference between Mr. DE LUC'S zero 69°.32, and the obſerved and reduced temperatures reſpectively, we ſhall have, in the firſt caſe, 12200; and in the laſt, 12390 to 1, for the ratio of the weight of quickſilver to air; either of which exceeds very conſiderably 11232, which hath been aſſigned to them.

With regard to the obſervations on the Dole, the defect is $\frac{81.4}{1000}$, anſwering to the obſerved temperature of 66°.6, and which is only reduced to 65°.2. On this great height, the ratio of the weight of quickſilver to

air(m) is that of 12595 to 1. Mr. DE LA CAILLE's obſervations at the Cape of Good Hope, annexed to the table containing Mr. DE LUC's, give a defect of $\frac{58.7}{1000}$, when the temperature ſeems to have been about 58°, in which caſe quickſilver was 11687 times heavier than air.

Now if, from the aggregate of theſe obſervations, the ſame method be adopted, as was made uſe of in the Britiſh, for finding the zero of the ſcale, we ſhall have it as follows:

	By Mr. DE LUC's equation for the air and obſerved temperature.			By the manometrical equation and reduced temperature.		
	°	°	°	°	°	°
Coldeſt of the morning obſervations, from —	25.2 to	30.5 at	33.7	from 25.2 to	30.5 at	33.12
Coldeſt of the ordinary obſervations,	27.1 —	41.9 —	38.7	—— 26. —	35. —	32.97
Hotteſt of the ordinary obſervations,	76. —	84.5 —	36.2	—— 73.5 —	77. —	36.32
On the Dole, —	59.2 —	71.5 —	27.6	—— 58. —	70. —	32.
Light houſe of Genoa,	75. —	79. —	26.	—— 75. —	79. —	33.40
DE LA CAILLE's,	—	58. —	30.	—— —	58. —	33.35
Zero at	—	—	32.03	Zero at		33.52

(m) It will even be found, though the calculations are not inſerted in the table, that the hotteſt of Mr. DE LUC's morning obſervations, June 8th, 1758, at the 15th ſtation, anſwering to the mean temperature of 57°.5, and which I conſider as the beſt, becauſe no reduction is neceſſary for the expoſition of the thermometer, agree with the manometrical experiments, in requiring a greater equation than is wanted in extreme temperatures: for in this caſe, the mean of two obſervations gives a defect of $\frac{65.5}{1000}$ for 25°.5 above freezing, which is 2.57 for each degree; the ratio of the weight of quickſilver to air being that of 12196 to 1.

From

From the mean of theſe obſervations, though the reſults are irregular among themſelves, it appears ſufficiently evident, that if the morning obſervations on Saleve had been retained, inſtead of being made exceptions from the rule, the zero of the ſcale would have deſcended about 8°; *viz.* from 69°.3 to 61°.4 of FAHRENHEIT, ſuppoſing always the equation 2.1 for each degree of temperature, and the French toiſe, as the ſtandard meaſure, to have been adhered to: for the French toiſe bears to the Engliſh fathom, the proportion of 106575 to 100000; wherefore $\frac{6575}{106575} = \frac{61.69}{2.1} = 29°.4 + 32° = 69°.4$, denotes the relative poſitions of the two zeros, the intermediate equation $\frac{61.69}{1000}$ being to be ſubtracted, when the toiſe is made uſe of. But it hath been ſhewn, that the mean expanſion of air is really greater, for ſuch temperatures at leaſt as the barometer can be applied in, than what Mr. DE LUC ſuppoſed it, in the proportion of 245 to 210; whence it follows, that $\frac{61.69}{2.45} = 25°.18 + 32° = 57°.18$, will denote the relative poſitions of the two zeros; which, inſtead of almoſt 30°, are only diſtant from each other a little more than 25°.

From what hath been ſaid it is eaſy to ſee, that in calculating heights according to Mr. DE LUC's rule, when the temperature of the air is below his zero, which we may

may take at 40°, the English measure being used, the common error in the result will be equal to the sum of the two equations, $2.1+2.45=4.55$ for each degree; which amounts to $\frac{36.4}{1000}$ parts for the 8° that the zero is too high. Above 40°, the former error $\frac{36.4}{1000}$ will be augmented by the difference of the equations for each degree that the temperature is above his zero, *viz.* $2.45-2.1=\frac{0.35}{1000}$. In either case it is to be observed, that the progressive rate of equation for the heat of the quicksilver is not here taken into the account; because it will not produce any material difference, unless one barometer is much hotter than the other, at the same time that their vertical distance is very great. Thus the 32d degree of FAHRENHEIT, or freezing temperature, which is fundamental in all thermometers, happens, somewhat remarkably, to be the zero of the scale, when the English fathom bears such proportion to the modulus of the common logarithms, as that their difference, in computing heights by the barometer, brings out the result in fathoms. No other proportion of a measure will do it: for if we suppose twenty-four of different lengths, between ours and the French toise, each surpassing the other by $\frac{263}{100000}$ of that toise, the zero of the scale, in computing heights by these measures respectively, will ascend a single de-

 gree

gree on each; and the French toife being the 25th, will have its zero nearly at the 57th degree: about which temperature the expanfion of air appears, from the experiments, to be at its maximum. From that point, therefore, the equation will diminifh both ways, though by a quicker progreffion for condenfation, than it doth for dilatation.

Having thus compared, in a general way, the refults of the Britifh obfervations with thofe of Mr. DE LUC, pointed out what feem to be the chief caufes of the conftant defect found in his rule, and thereby obtained, it is hoped, fome corrections tending to improve the theory of the barometer, when applied to the meafurement of heights in middle latitudes; it remains to fhew the principles, whereon the table for the equation of the air hath been conftructed. Previoufly however to this, it may be proper to compare, with as much brevity as poffible, thefe obfervations, with others that have been made towards the Pole and at the Equator: from which it will appear probable, that the rule which anfwers in middle latitudes, will not in the frigid and torrid zones.

In 1773, Captain PHIPPS, now Lord MULGRAVE, commanding two of his Majefty's fhips then fent on difcoveries towards the North Pole, meafured geometrically, with great care, the height of a mountain in Hakluyt's Ifland

near

near Spitzbergen, and found it to be 1503 feet above the level of the sea. On the morning of the 18th of August, the following observations, at the sea-shore and top of the mountain, were made with a single barometer, wherein the quicksilver had not been boiled.

At 6 h. A.M. Barometer at the shore, — —	30.040	therm.	50°
7 h. 45′ A.M. Ditto at the top of the mountain,	28.266	—	42
8 h. 45′ A.M. Ditto at the top of ditto, —	28.258	—	42
11 h. 45′ A.M. Ditto at the sea-shore, —	30 032	—	44

Whence we have the following computations, equated for the times corresponding to the two observations at the top.

7 h. 45′ A.M.	30.038 28.266	46°—046=29.992 42 —031=28.235	=1573	+70. =44.5	43° 41	42°	Quicksilver to air as 10224 to 1.
8 h. 45′ A.M.	30.036 28.258	45°—043=29.993 42 —031=28.227	=1581.3	+78.3 =49.5	43 41	42	
		Mean	1577.1 = +$\frac{47}{1000}$				

From these observations it appears that, instead of the usual equation $\frac{24.5}{1000}$, to be added to the logarithmic result, in order to obtain the true height in Britain, when the temperature is 42°, there is an excess of $\frac{47}{1000}$: and, instead of the usual ratio of the weight of quicksilver to columns of air, of equal altitude and temperature in Britain, namely about 11200, we have that of 10224 to 1.

 Thus

Thus air at Spitzbergen ſeems to be ſpecifically heavier, than that affected with the ſame heat and preſſure in the middle latitudes: whence it follows that, inſtead of 32° which is found to be the zero of the ſcale about the middle of the temperate zone, we ſhall have $\frac{47}{245} = 19°.2 + 42° = 61°.2$ for the zero at Spitzbergen, within 10° of the North Pole.

It is much to be regretted, that the French academicians, when employed in meaſuring the degrees of the meridian in Peru, were not ſupplied with better barometers, and that they made not obſervations at correſponding times; ſince the ſcene of their operations was undoubtedly preferable to any other on the ſurface of the globe, for determining many curious points with reſpect to the modifications of the atmoſphere in the torrid zone: nevertheleſs, by attending diligently to what Mr. BOUGUER (n) hath told us, of the ſteadineſs of the barometer

(n) He ſays, that at the South Sea, REAUMUR's thermometer, in the morning before Sun-riſing, ſtood at 19°, 20°, or 21°; and in the afternoon at 26°, 27°, or 28°. The reſpective means correſpond to $76°\frac{1}{2}$ and $92°\frac{1}{2}$ of FAHRENHEIT, and make the mean heat of the day $84°\frac{1}{2}$. At Quito the temperature continued at 14° or 15°, anſwering to $65°\frac{1}{2}$ of FAHRENHEIT. At the ſummits of Coraçon and Pichincha, the thermometer ſtood in the morning ſeveral degrees below freezing, and varied 17° in the heat of the afternoon; whence the mean temperature at theſe higheſt ſtations, would probably be about $43°\frac{1}{2}$ of FAHRENHEIT. He farther ſays, that in the torrid zone, whatever may be the mean

meter throughout the year; the uniformity of the mean temperature in every affigned ftation; and his mode of computing, by means of the tables of common logarithms, the altitudes of the Cordillero mountains above the valley that extends itfelf between them; it will be no difficult matter to difcover, nearly at leaft, what fort of equation became neceffary; and what were the relative weights of quickfilver and air of the mean temperature, not only in that high region of the atmofphere, but alfo at the level of the fea.

Thus, by infpecting the table of computations, it will appear, that columns of air, whofe bafes were removed fix or eight thoufand feet from the level of the fea, and whofe heights equalled that diftance, when the temperature was 55° of FAHRENHEIT, as determined from the mean between the coldeft of the morning and hotteft of the afternoon, the mean logarithmic defect was only $\frac{36.3}{1000}$: whereas, in meafuring heights near the level of the fea, in middle latitudes, the common equation for that temperature is about $\frac{57}{1000}$. The mean ratio of the weight of quickfilver to air, on thefe long columns comprehended refpectively between Carabourou and Quito, and the

mean heat in any affigned ftation, it continues uniformly the fame throughout the year. In this rough eftimation of the temperature in Peru, it feemed unneceffary to examine, whether the true thermometer of REAUMUR was ufed or not; as it could produce no material difference, except at the very hotteft ftations.

fummits of Pichincha and Coraçon, is that of 16793 to 1. On the altitude of 1534 feet, intercepted between Carabourou and Quito, which fhort fection of the column is about half-way between the level of the fea, and the fummits of the Cordilleros, the mean temperature being 66°, the ratio is that of 15089 to 1: hence it feems probable, that quickfilver would have to the different fections of the general column of air, comprehended between the fea and the top of Coraçon, nearly the following ratios:

	Temp.	
At the level of the South Sea,	$84\frac{1}{2}$	13100 to 1
Half-way from thence to Carabourou,	$75\frac{1}{2}$	14100
At Carabourou, — —	$66\frac{1}{2}$	15100
Half-way from thence to Coraçon,	55	16100
At the fummit of Coraçon, —	$43\frac{1}{2}$	17100
Whereof the mean is, —	65	15100

Mr. BOUGUER tells us, that the barometer in the torrid zone varies not at the fea-fhore above two and a half, or at moft three lines throughout the whole year. At Popayan, its variation is only a line and a half; and at Quito a fingle line. Now let us fuppofe, that an altitude had been meafured with the barometer at the level of the

the South-sea, where the descent of quicksilver at the upper station was exactly an inch in the mean heat of the day, answering to $84^{\circ}\frac{1}{2}$. On the former supposition of the weight of quicksilver to air, the height would be 13100 inches or 1091.7 feet.

$$\text{Hence} \left\{ \begin{matrix} 29.930 & 84\tfrac{1}{2}-169=29.761 \\ 28.930 & 84\tfrac{1}{2}-169=28.761 \end{matrix} \right\} = 890.6 \text{ feet;}$$

the logarithmic result, which is defective 201.1, or nearly $\frac{226}{1000}$ parts. Now this equation being divided by 2.45 the mean expansion of air, we have nearly 92° for the difference between $84^{\circ}\frac{1}{2}$, the temperature of the observation, and the zero of the scale, which reduces it to $-7^{\circ}\frac{1}{2}$ of FAHRENHEIT. If it should be thought that I have supposed the air to be too light at the level of the sea under the equator, let it be taken to quicksilver only, at a mean between 13100 and 12672, which seems to have been the ratio of their weights at Genoa, when Mr. DE LUC's temperature was 79°, and we shall have 12881 inches, or 1073.4 feet of air, for the counterpoise to the inch of quicksilver in the barometer: hence $1073.4-890.6=\frac{182.8}{890.6}=\frac{205}{2.45}=83^{\circ}.7$, will denote the number of degrees that the zero of the scale would, in that case, be below the temperature of the air, [illegible]gs it to within less than a degree of the cy[illegible] [illegible]NHEIT. But in middle latitudes the zero [illegible] [illegible]d the

equation, applicable at the level of the ſea for the heat of $84°\frac{1}{25}$, is at moſt only $\frac{132}{1000}$ inſtead of $\frac{205}{1000}$.

Mr. BOUGUER found, that the rule which his experience had furniſhed him with, for computing heights with the barometer between the ranges of the Cordilleros, namely, that of deducting $\frac{1}{30}$th part from the number of toiſes expreſſed by the logarithmic differences, which agrees nearly with the equation $\frac{36.3}{1000}$ which I have made uſe of in the table of computations, would not anſwer when he came to apply it at the level of the ſea. He tells us, indeed, that the elaſticities of the air, above and below, are there, as well as in Europe, exactly proportionable to its condenſations; and even, that the intenſity of the elaſtic force, or ſpring of the air, is every where equal in all places of the torrid zone that are conſiderably elevated. The real condenſations in each place are proportionable to the weights of the ſuperior columns cauſing the compreſſion; theſe condenſations being in geometrical, the heights are in arithmetical progreſſion. But below the ſame law doth not take place; becauſe the intenſity of the elaſtic force is really conſiderably leſs at the level of the ſea, than it is at one or two hundred toiſes above it, notwithſtanding the effect of the heat, which ſhould render it greater. It is to be obſerved, that Mr. BOUGUER hath not given us the obſervations whereon he

he founded this last deduction; and his note on the text, which I apprehend, nevertheless, conveys his true meaning, is contradictory to it: for there he says, that the dilatation occasioned by the heat throughout the day, changed the distribution of the weight with regard to all the places situated within the Cordilleros, as well as on other mountains, and made the lower sections of the columns contain less and the upper sections more air, than they should have done, had it been a perfectly elastic fluid.

Having now mentioned all the barometrical observations that have come to my knowledge, tending any way to throw light on this very intricate subject, it remains to sum up, from the whole, the general principles whereon I have proceeded in constructing the table of equation for the heat of the air.

It will be remembered, that I have more than once remarked, that in the British observations, when the temperature was 52°, the defect was $\frac{49 \text{ or } 50}{1000}$, the lowermost barometer standing at or near the level of the sea; but in the observations on Tinto, a considerable hill appertaining to the third class, whose base is elevated 700 feet above the level of the Clyde at Glasgow, when the temperature was 52°, I found the equation to be little more than $\frac{45}{1000}$. Again, these two facts being compared with

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the aggregate reſult of Mr. DE LUC'S obſervations, where the lowermoſt barometer ſtood about 1300 feet above the ſea, the equation for the ſame temperature ſeemed not to exceed $\frac{42}{1000}$. Laſtly, theſe circumſtances lying confronted with the reſults of Mr. BOUGUER'S obſervations, where the lowermoſt barometer ſtood from 6000 to 8000 feet above the ſea, the mean equation for 55° was only $\frac{36.3}{1000}$, which gives $\frac{34}{1000}$ for the heat of 52°. Now theſe Peruvian obſervations, which I believe to be exceedingly good from the ſteadineſs of the barometer in that part of the world, being ſubſtituted in lieu of thoſe not yet obtained in our own quarter of the globe, there ſeemed to me to be a neceſſity for concluding, that the equation for middle latitudes, with any aſſigned temperature above or below the zero of the ſcale, diminiſhed as the height of the place above the ſea increaſed; which conſequently implied, that the magnitude of the logarithmic terms increaſed faſter than the dilatations of the air. But when the compariſon was carried yet farther, and the obſervations in Peru and at Spitzbergen were fairly brought into one view, there appeared to be ſufficient grounds for ſuſpecting, if not abſolutely for concluding, that there could be no fixed zero for the ſcale depending on the temperature of the air; but that it would change with the denſity of the atmoſphere appertaining to the

latitudes,

latitudes, climates, or zones of the earth, where the observations were made. On this supposition it was natural for the mind to form to itself some general hypothesis, which might serve to account for the appearances; and the first that presented itself was the following: that the atmosphere surrounding our globe might possibly be composed of particles, whose specific gravities were really different; that the lightest were placed at the equator; and that the density of the others gradually increased from thence towards the poles, where the heaviest of all had their position [o].

It is a well known and established fact, that in the middle latitudes, a North or North-east wind constantly raises the barometer, and generally higher as its continuance is longer. The contrary happens when a South or South-west wind blows; for I believe it is commonly lowest when the duration and strength of the wind from

(o) It was suggested by Dr. GEORGE FORDYCE, that equatorial and Greenland air might be brought bottled up, and weighed in this country in air of the respective temperatures, by means of a curious balance whereof he is possessed, in order to see whether any difference could be discovered in their specific gravities. A thought of the same kind, but more easily put to experiment, occurred to Lieutenant GLENIE, of the Royal Artillery, namely, that of weighing equatorial and polar sea-water. To this gentleman I am obliged for his assistance in part of the manometrical experiments, as well as in several of the computations.

 that

that quarter have been the greateſt. Thus the North-eaſt wind, by blowing for any length of time, brings into the middle latitudes a maſs of air heavier than that which naturally appertains to the region, and raiſes the barometer above its mean height. The continuance of a South-weſter carries off the heavy air, depoſits a much lighter body in its ſtead, and never fails to ſink the barometer below its mean height: hence, in the middle parts of Europe, there is a difference of about two inches and a quarter between the higheſt and loweſt ſtates of the barometer. But ſuppoſing it to be only two inches, the difference of preſſure ſtill amounts to $\frac{1}{15}$th part of the whole weight of the atmoſphere. Now it is evident from the Peruvian obſervations, that the greateſt fluctuation of the barometer, which is at the level of the ſea, doth not exceed 0.226 of an inch, or $\frac{1}{133}$d part of the whole preſſure; and if the change ſhould be no greater at the poles, which I think not improbable, it follows, that the meaſurement of heights by means of the barometer, in middle latitudes, will be more precarious and uncertain than in the torrid and frigid zones.

Such in general were the firſt ideas which the compariſon of the operations of the barometer with the effects

of

of the North-east and South-west wind (p) on that instrument, suggested with regard to the different densities of the atmosphere in the different zones of the earth.

But since the experiments on the expansion of moist air have shewn its elasticity to be so much greater than that which is dry, I apprehend, that the simple principle of heat and moisture may suffice to account for all the phenomena. Thus it is universally admitted, that there is a greater degree of humidity and heat in the air, near the earth's surface, than there is in the higher regions of the atmosphere. The elasticity or expansion of the lowermost section (q) of every column of air, whether long

(p) I have been well informed, that in China the North-west wind raises the barometer most, and is highly electrical; it is at the same time the driest and the coldest; and at Canton, under the Northern tropic, there is frequently ice. On the East-coasts of North America the severity of the North-west wind is universally remarked; and there can scarcely be a doubt, that the inhabitants of California, and other parts on the West-side of that great Continent, will, like those on the West of Europe, feel the strong effects of a North-east wind. The extraordinary dryness and density of the wind from the North pole, seems therefore to be occasioned by its passing over the Continent of Europe and Asia on one side, and that of North America on the other. Those who live on the East and West-coasts of South America, will find the driest and coldest winds come to them respectively from the South-west and South-east. As the winds seem to be colder, drier, and denser, in proportion to the extent of land they pass over from the poles towards the equator, so they appear to be more moist, warm, and light, in proportion to the extent of Ocean they pass over from the equator towards the poles. Hence the humidity, warmth, and lightness, of the Atlantic wind to the inhabitants of Europe.

(q) Mr. DE LUC seems to have suspected something of this kind towards the end

long or ſhort, will conſequently be greater than the uppermoſt ſection of it; for the heat, by diſſolving the moiſture, produces a vapour lighter than air, which mixing with its particles, removes them farther from each other, increaſes the elaſticity of the general maſs, and diminiſhes its ſpecific gravity comparatively more than it doth that of the ſection immediately above it, where there is leſs heat and leſs moiſture. Hence I infer, that the equation for the air, in any aſſigned vertical, will gradually diminiſh as the elevation of the place above the ſea increaſes, and that it will vaniſh at the top of the atmoſphere. This is in ſome reſpect confirmed by the experiments on the expanſion of rare air; for from them it appears, when the particles are very far removed from each other, by a great diminution of preſſure, as is undoubtedly the caſe in the higher regions of the atmoſphere, they loſe a great part of their elaſtic force. Thus the equation, anſwering to any particular temperature, above or below the zero of the ſcale, at different heights above the ſurface, will, I apprehend, be expreſſed by the ordinates to a curve of the hyperbolic order, whoſe cur-

end of his 8th chap. *ſur les difficultés à vaincre:* and in that which follows, he gives proofs of the lightneſs of vapours with regard to air, ſaying, that they point out fire to be their vehicle. He afterwards quotes NEWTON with reſpect to the lightneſs of a humid atmoſphere compared with one that is dry.

vature may be ſuppoſed to change faſt near the ſurface of the earth, and differ inſenſibly from a ſtraight line at great heights above it.

With regard to the latitudinal equation, the ſame principle of heat and moiſture ſeems to make it probable, that ſuch will become neceſſary in operating with the barometer; for it is well known, that there is a great degree of humidity in the air between the tropics; and, on the contrary, that the polar atmoſpheres are very dry. The heat and moiſture being greateſt at the equator, there the elaſticity or equation will likewiſe be the greateſt at the level of the ſea; and the zero of the ſcale will neceſſarily deſcend to a lower point of the thermometer, than that to which it correſponds in middle latitudes. As the elaſticity of the air at the level of the ſea, or equal heights above it, with the ſame degree of heat, will always be proportionable to the quantity of moiſture diſſolved in it, therefore it will gradually diminiſh from the equator towards the poles; that is to ſay, the zero of the ſcale will aſcend in the thermometer, coincide with the 32d degree in the middle latitudes, and, in its motion upwards, will give the equation to be applied with the contrary ſign in high latitudes. Hence I infer, that every latitude, climate, or zone, will not only have its particular zero, but alſo its particular curve, whoſe ordinates will

will always meaſure the equations applicable in the reſpective ſituations. The equatorial curve will probably change the faſteſt, and the others become gradually flatter, as they approach towards the poles, where the greater, but more uniform, denſity of the atmoſphere may occaſion it to differ little from a ſtraight line. I apprehend, however, that even at the pole ſome ſmall diminution might be found to take place in the equation, was it poſſible, in that region, to prove it by experiments at a ſufficient height above the level of the ſea.

The table of the equation, depending on the heat of the air, annexed to this paper, is conſtructed for middle latitudes. It extends to temperatures from 12° to 92° of FAHRENHEIT; and for ſituations ſo greatly elevated above the ſea, as to make the mean barometer between the two ſtations ſtand no higher than 19 inches. As the equation correſponding to the lower parts of the atmoſphere, contained in the right-hand columns, will come more frequently into uſe than that appertaining to the higher regions, comprehended in thoſe on the left; therefore, in the firſt, it is given for every half; and in the laſt only, for every whole inch of deſcent of quickſilver in the tube.

The equation found in the column of 29 inches, correſponds exactly with the expanſion of air reſulting from

the manometrical experiments; and the ratio of diminution, in the temperature of 52°, hath been taken from the Peruvian obſervations, ſuppoſing it to decreaſe uniformly $\frac{2}{1000}$ on each inch, or $\frac{16}{1000}$ on the eight inches between 29 and 21. For the ſake of ſimplicity, as well as from the want of ſufficient data for aſcertaining the lengths of the ordinates of the curve, the arithmetical hath been preferred to any progreſſive diminution that might have been adopted, though by this mode the reſults would have agreed better with ſome of my own, as well as Mr. DE LUC's obſervations. In each of the columns the equations for particular temperatures, compared with that for 12° or 92°, are reciprocally proportionable, ſo that the maximum of the rate always correſponds to the ſpace between 52° and 72°, as indicated by the manometer. It will be obſerved, that though the equation in the table is only given for every 10° of difference of temperature, yet, by the intermediate rates for ſingle degrees in the columns reſpectively, and the ratio of diminution for the height of the mean barometer above the ſea, expreſſed in that towards the right-hand, the equation for any particular temperature may be readily obtained. The application of this table makes the third part of the rule, for meaſuring heights with the barometer. When the mean temperature of the column

of

of air is above 32° of FAHRENHEIT's thermometer in the ſhade, add the equation correſponding to the temperature and height of quickſilver in the mean barometer to the logarithmic altitude; when below 32°, ſubtract the equation from the logarithmic altitude; the ſum in the firſt caſe, and difference in the laſt, gives the real height.

Beſides the table of equation for the air, adapted to the meaſurement of the greateſt acceſſible heights the barometer can poſſibly be applied to in middle latitudes, I have annexed, for the uſe of thoſe who may prefer ſimplicity, and ſtill doubt of the vertical diminution, a thermometrical ſcale of the equation, ſuited to Engliſh and French meaſures, with their reſpective thermometers. It will readily be conceived, that the diviſions, expreſſing the 1000th parts in this ſcale (r), are unequal, ſince they follow the inverſe ratio of the thermometrical compared with the manometrical degrees. Where theſe laſt are the greateſt, as between 52° and 72°, the diviſions expreſſing the equation are the ſmalleſt, becauſe a greater

(r) Any ſcale of this kind, unleſs it had been mechanically divided by a mathematical inſtrument-maker, could not be rendered very exact; and it may be expected, that the imperfections in the original will be augmented in copying by the engraver, notwithſtanding the utmoſt care on his part: wherefore, on the left-hand ſide of the plate, I have annexed the number of degrees and decimal parts of FAHRENHEIT, below the temperature of 91°.88, correſponding to every $\frac{10}{1000}$th parts of the equation, by which means the unequal ſcale may, at any time, be divided with ſufficient accuracy.

number

number of them correspond to the same thermometrical space. When the height is required in fathoms, the zero of FAHRENHEIT corresponds to −71.72, and the boiling point to +412.49: the sum of the two equations 484.21 is the actual expansion of common air from the heat of 212°. When the French toise is made use of as the measure, the zero of the scale hath been shewn to coincide with 57°.18 of FAHRENHEIT, or $+11^{\circ}\frac{1}{4}$ of REAUMUR. The negative equation 134°.72 answering to $-14^{\circ}\frac{1}{4}$ of REAUMUR, and the positive 349°.49 corresponding to +80°, or the boiling point, being added together, make again 484.21.

In order to convey a more distinct idea of the effect which heat produces in the dilatation of different kinds of air, compared with quicksilver, along with the scale for the equation I have placed another, expressing the actual and relative expansions, resulting from the mean of the experiments, for every 20° of difference of temperature. This scale is intended to give a comparative view of the manometrical with the thermometrical spaces, mentioned in the second section.

I shall now close this paper, which hath already greatly exceeded the limits I wished to have been able to prescribe to it, with a few remarks on the error of the rule, perceivable in the tables of computation, and the measures

fures that fhould, in my opinion, be taken to bring the theory of the barometer to a ftill greater degree of perfection, fuch as I believe it to be really capable of.

By infpection of the tables containing the computations of the Britifh obfervations, it will be feen, that the error of the rule is in general very fmall. In the London clafs it is greateft on Shooter's-hill, making the height five feet too little. In thofe at Taybridge, one of the obfervations on Schihallien gives a defective refult of $29\frac{1}{2}$ feet; but this is eafily accounted for, as it certainly arofe from the fhort time given to the barometer to lofe the heat it had acquired in carrying upwards, thofe deftined to obferve at the fummit arriving there too late, that is to fay, towards the expiration of the whole hour which the inferior barometer had been obferved in. One of the obfervations on Carmichael-hill, though a fmall height, is defective eight feet, which I afcribe to the South-weft wind and humidity of the air. From the fame caufe I would account for the uniform defect in the firft part of the Edinburgh obfervations: in the laft part of thefe, the circumftances having changed, the error hath the contrary fign. In the Linhoufe clafs of obfervations, the fame caufe of defect appears on the 1ft of December, 1775, and on the 20th of November, and 17th of December, 1776. The only refult which I

confider

consider as very irregular, and do not pretend to account for, is that for the height of Moel Eilio, a hill situated between Carnarvon and Snowdon: the real altitude 2371 feet, is exceeded by the barometrical result 21 feet, though the circumstances were such as, in other cases, generally make it fall short.

At the bottom of the tables of computations I have occasionally substituted Mr. DE LUC's equation for the air, in calculating one or more of the greatest heights, that the difference between the two methods might become more obvious. Thus the first observation on Schihallien is defective $67\frac{1}{2}$ feet; the mean of those on Tinto 29 feet; Moel Eilio 41 feet; and Snowdon 81 feet.

With respect to the results that the rule produces on Mr. DE LUC's heights it will be observed, that it answers very well in the cold observations, which, with his rule, were often defective 60 or 70 feet; but gives too much in those that are hot. If, however, the whole of these hot observations had been included, the apparent error would have been less; for the mean defect was taken at $\frac{96}{1000}$ for the hottest temperature, whereas it sometimes amounts to $\frac{110}{1000}$. On the height of the Dole the rule errs in defect; and on the mean of Mr. DE LA CAILLE'S observations, at the Cape of Good Hope, it exceeds the truth. By substituting Mr. DE LUC's equation for the air,

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in the computations of the Dole and Table-hill, the refpective refults are defective 96 and 62.6 feet.

To the Britifh obfervations a table is annexed, containing the barometrical computations of altitudes not yet determined geometrically. In the chief part of thefe the inferior barometer ftood at Belmont-caftle, the feat of the lord privy-feal for Scotland, by whofe directions the correfponding obfervations were made. This table likewife comprehends Mr. BANKS's obfervations in 1772, for the height of the South-pap of Jura, above Freeport in the ifland of Ifla, and thofe he made the fame year, to obtain the height of Mount Hecla, above Hafniford in Iceland.

Laftly, it is to be obferved, that in the application of the table, the equation found in the columns $29\frac{1}{2}$, 30, and $30\frac{1}{2}$, will never come into ufe, except in the meafurement of fhort columns of air, whofe bafes ftand at, or not much above, the level of the fea[(s)]. In an ifland, whofe

(s) Having been accuftomed, from the beginning, to call the ftation of the inferior barometer the place of obfervation, and to fuppofe the mean height of its quickfilver to denote the elevation of the place above the fea, for the fake of fimplicity I adapted the formula to the height of quickfilver in that barometer, and made all the computations in the tables accordingly. But it having been fuggefted to me, firft by Sir GEORGE SHUCKBURGH, and afterwards by Mr. DE LUC, that this mode, though the eafieft, was not ftrictly accurate, nor confiftent with the principles whereon a vertical diminution of the equation for the

whose climate is so very variable as that of Britain, settled weather should be chosen as the best time for observations. With any sudden fall of the barometer, in any assigned station below its mean height, it is apprehended that the rule will have a tendency to give defective results; and the contrary should happen when, from the increased weight of the atmosphere, it rises much above the mean height.

From what hath been said in the course of this paper, it will be perceived, that though the error of the rule is in general very small, yet now and then such irregularities do occur as plainly shew, that something still remains to be done, in order to perfect the theory of the barometer.

The existence, or otherwise, of a latitudinal equation being a point of the greatest consequence, should be determined with so much care as to leave no doubt remaining on that head. And as this can only be effected by differences that are extremely obvious, the observations for that purpose should be made at the equator, and as near as possible to the poles[(t)]. Peru is no doubt the best situation

the heat of the air was founded, I have since changed it to the mean barometer, or middle of the column of air intercepted between the two stations. In this way all the great heights have been re-computed: the smaller altitudes, not being sensibly affected by the alteration, continue as at first.

(t) Some idea may be formed what altitudes on the surface of the globe are accessible

fituation on the globe for conclufive equatorial obferva-tions; but as it would be found very difficult to carry any fcheme of that kind into execution, fuch as may be more eafily obtained in our Weft India iflands, which have the higheft mountains, would be very fatisfactory with refpect to the expanfion and weight of moift air, at different heights above the furface. At the tops of the mountains in the torrid zone, the obfervations would always be fufficiently cold; but it would be of ufe likewife, to have the coldeft poffible at the level of the fea, under or near one of the tropics, when the Sun was in the other.

With regard to obfervations in the frigid zones, Spitzbergen feems to be as proper a fituation as any; though others may no doubt be found in the Northern parts of the Ruffian empire: and it is prefumed, that the Peterfburg academy would direct the experiments to be made.

acceffible to man, by confidering the height above the fea of the inferior line of perpetual fnow. In the middle of the torrid zone it appears, from Mr. BOUGUER's obfervations, to be elevated 5201 yards, and 4476 about the tropics. In middle latitudes there is everlafting fnow on the mountains at the height of 3300 yards. In the latitude of 80° North, Lord MULGRAVE found the inferior line of fnow to be only about 400 yards above the fea: whence we may conclude, that the furface of the earth, at the pole itfelf, is for ever covered with fnow.

The Peak of Teneriffe, Ætna, the mountains of Auvergne and Rouſſillon, as well as Hecla in Iceland, are all very proper for obſervations in intermediate latitudes.

Within the iſland of Great Britain, Ben Nevis ſeems to be the beſt mountain for barometrical obſervations, becauſe of its great height, its vicinity to the ſea, and that there is very good ground cloſe to its foot (which is rarely the caſe in the Highlands) for the meaſurement of the baſe that would be made uſe of in the geometrical operations.

One of the chief cauſes of error in barometrical computations, I apprehend, proceeds from the mode (though ſimplicity is in its favour) of eſtimating the temperature of the column of air from that of its extremities, which muſt be faulty [(u)] in proportion as the height and difference of temperature are great. Where very accurate concluſions are expected, ſimultaneous obſervations, at different times of the day, and different ſeaſons of the year, ſhould be made with ſeveral barometers, placed at different heights, each accompanied with a thermometer and manometer. By this method, the progreſſion of temperature, as well as the law of diminution of the equation, from the poſition of the inferior barometer above the ſea (if ſuch diminution doth really take place)

(u) This is taken notice of by Mr. DE LUC.

would

would be obtained with certainty. Suppofing, for inftance, Ben Nevis was divided into four fections, five barometers, with as many obfervers, would be neceffary. This number may feem great, but the expence of people employed in that way would be inconfiderable. And if it fhould be judged proper, there could not furely be any great difficulty in providing reafonable accommodation for an obferver, who fhould live a whole year at the top of the mountain, while another made correfponding obfervations below.

But the perfecting of the theory of the barometer is not the only advantage that would accrue from a combination of thefe obfervations; for, while they were carrying on in different climates, or zones of the earth, good opportunities would offer of determining the refractions, as well as the force of gravity and figure of the globe, from the vibrations of the pendulum.

The mean expanfion of common air is already found to be greater than what was formerly fuppofed; wherefore the mean refraction will be altered proportionably. And fince the expanfion of moift air is found to be fo much greater than that of common air, a larger field for inquiry and inveftigation is now laid open.

With refpect to the experiments with the pendulum, Mr. BOUGUER feems to have been the only perfon, fo far

as

as I know, who hath taken the denſity of the medium in which it performed its vibrations into the account, and given us its length at the equator in vacuo. But if we are to judge of the denſity of the air in the frigid zone from the barometrical obſervations at Spitzbergen, the pendulum there muſt have loſt ſo much of its weight, as to have leſſened conſiderably the number of vibrations below what they would have been in vacuo, in the ſame temperature. Having conſidered the effect that this would produce, I collected the beſt experiments that have hitherto been made with the pendulum into one view, and having applied the equation that the denſity of the air, in which they ſeverally vibrated, ſeemed to require; I found from computation, that the ratio of the diameters of the earth is (as Mr. BOUGUER ſuppoſed it) nearly that of 178 to 179, inſtead of 229 to 230, as eſtimated by Sir ISAAC NEWTON, and which agrees very nearly with the mean reſult from the meaſurement of the degrees of the meridian. The experiments with the pendulum are ſo ſimple and eaſy, may be repeated ſo often in all ſituations, and are ſo much more conſiſtent with each other, than the meaſured lengths of degrees of latitude, that it appears to be incomparably the beſt method for determining the figure of the earth. And if it ſhould really be found ſo flat a ſpheroid as the pendu-

 lum

lum feems to make it, both parallaxes and refractions, will require correction.

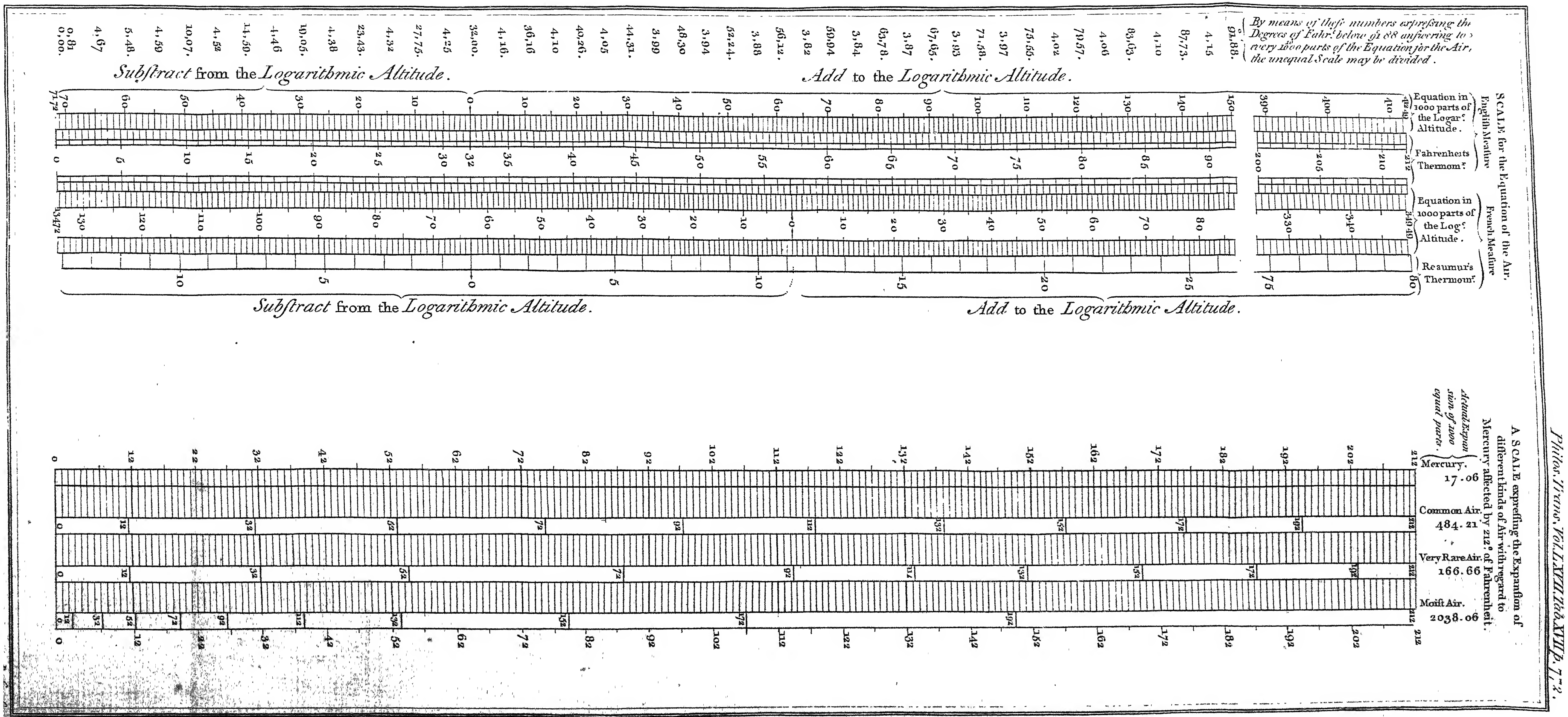
Upon the whole, though I wifhed to be concife in the recital of the experiments and obfervations contained in this paper, yet I found it neceffary at the fame time to be explicit. Some of them were either entirely new, or managed in a different manner from what they had formerly been. This forced me into a comparifon of many minute circumftances attending the operations, and to a tedious, though neceffary, combination of the various refults. Without taking a comprehenfive view of the whole matter, and ftating every thing with fairnefs and candour, I could not convey to others the ideas I entertained of it myfelf; nor enable them to judge, how far I had been juft in the conclufions already drawn, or confiftent in my fuppofitions concerning fuch points as are yet left doubtful. If I have been obliged to differ from Mr. DE LUC, it is becaufe the Britifh obfervations, as well as his own (confidered by their extremes) feem to authorize it: he is himfelf too candid to fuppofe, that I have had criticifm in view, or indeed any other object, than that of contributing my mite towards the difcovery of the truth, from the very good foundation which he hath already laid for it. I am aware it may be alledged, that I have rendered the theory of meafuring heights by

the barometer ſo much more complicate and difficult, as perhaps to deter others from applying it to uſeful purpoſes. To this I anſwer, that though it ſeem utterly impoſſible to render what is really intricate in its nature, extremely ſimple; yet that the beſt and ſureſt method of arriving at ſimplicity at laſt will, in the firſt place, be to aſcertain the limits of deviation of the rule, by a proper number of good obſervations, made in circumſtances and ſituations as different as poſſible from each other. In the preſent ſtate of the matter, I doubt not but the barometer will be found to give reſults ſufficiently near the truth for all ordinary purpoſes, the nicer buſineſs of levelling alone excepted. It is the only inſtrument by which the relative heights of places, in very great and diſtant tracts of country, can eaſily and ſpeedily be obtained, by the preſſure of the atmoſphere alone. The method of uſing it is attainable by all, requiring only a little habit, and ſome degree of attention to prevent the admiſſion of air into the tube. Few people are qualified for the tedious and very laborious operations of accurate geometrical meaſurements. Mountainous countries rarely afford baſes of ſufficient length, which, to avoid error, muſt be meaſured again and again with the utmoſt care. Inſtruments of the moſt expenſive kinds muſt be employed to take the angles; at the ſame time that a thorough knowledge of their uſe,

 and

and a ſcrupulous attention to their various adjuſtments, become indiſpenſably neceſſary. In ſhort, the facility of one method, compared with the other, is ſo exceedingly obvious as to need nothing elſe to recommend it as a ſubject very curious and uſeful, and therefore well worthy of the reſearches of philoſophers, till, by their united labours, it hath been brought to perfection.

N° I.



Philos. Trans. Vol. LXVII. Tab. XVII. p. 772.
By means of these numbers expressing the Degrees of Fahr. below 91.88 answering to every 1000 parts of the Equation for the Air, the unequal Scale may be divided.
Substract from the Logarithmic Altitude.
Add to the Logarithmic Altitude.
SCALE for the Equation of the Air.
English Measure
Equation in 1000 parts of the Logar. Altitude.
Fahrenheits Thermom.
French Measure
Equation in 1000 parts of the Log. Altitude.
Reaumur's Thermom.
Substract from the Logarithmic Altitude.
Add to the Logarithmic Altitude.
A SCALE expressing the Expansion of different kinds of Air with regard to Mercury affected by 212° of Fahrenheit.
Actual Expansion of 1000 equal parts.
Mercury. 17.06
Common Air. 484.21
Very Rare Air. 166.66
Moist Air. 2038.06

N° I. Computations of barometrical obfervations made on heights in and near London.

Geometrical heights of the ftations in feet.	Date of the obfervations, winds, &c.	Obferved heights of the inferior and fuperior barometers.	Temperature of the quickfilver.	Equation for the heat of the quickfilver.	Equated heights of the barometers.	Logarithmic refult in feet.	Logar. excefs or defect in ft. and alfo in 1000th parts.	Temperature of the air,		Equation by the rule in 1000th parts, and alfo in feet.	Refult by the rule,		Error of the rule in feet.	Ratio of the weight of quickfilver to air [illegible]
								Particular.	Mean.		Particular.	Mean.		
St. Paul's Church-yard North-fide, and iron-gallery over the dome, 281 feet.	1774, Dec. 1. 9h 27' wind N.W.	29.659 29.338	33°¾ 34	—006 —006	29.653 29.332	283.6	+2.6ft. = 9./1000	32°¾ 33¾	33°¼	+ 3.8/1000 = 1,1 ft.	284.6			
	Dec. 31. 11h 52' A.M. wind N.E.	30.187 29.864	35½ 34¾	—011 —009	30.176 29.855	279.5	— 1.5 = 5.5	33 34	33½	+ 4. = 1.	280.5	281.4	+ 0.4	10505
	April 22. 11h 55' A.M. E. wind.	30.136 29.839	50¼ 53	—060 —069	30.076 29.770	266.5	—14.5 =54.4	49 53	51	+47.5 =14.7	279.2	– –	– –	11056
Top of Paul's-ftairs, and the faid gallery, 324 feet.	1774, Ap. 22. 0h 50' P.M.	30.206 29.842	55¾ 53¾	—080 —071	30.126 29.771	308.9	—15.1 =49.	55¼ 53¾	54½	+55. =17.	325.9	– –	– –	10860
	Dec. 1. 10h 2' A.M.	29.717 29.344	37 35	—016 —009	29.701 29.335	323.1	— 0.9 = 3.	37¼ 34	35¾	+ 6.3 = 2.	325.1	324.6	+ 0.6	
	Dec. 31. 0h 22' P.M.	30.230 29.858	35½ 34¾	—011 —009	30.219 29.849	321.	— 3. = 9.3	34¾ 34¼	34½	+ 5.8 = 1.8	322.8	– –	– –	10565
Scotland-yard wharf, and Old Spaniard dining-room, 422 feet.	1774, Dec. 24. 10h 7' A.M. N.E. wind.	30.844 30.349	33½ 33¼	—005 —004	30.839 30.345	420.8	— 1.2 = 3.5	34¼ 33¼	33¾	+ 4. = 0.9	421.7	– –	—0.5	10251
Great Pulteney-ftreet, and the faid dining-room, 352 feet.	1774, Nov. 28. 9h 48' A.M.	29.684 29.287	35½ 34¾	—011 —009	29.673 29.278	349.2	— 2.8 = 8.5	35¾ 34¼	35	+ 7. = 2.4	351.6	– –	– –	10694
	Dec. 9. 0h 15' P.M. N.W. wind, fnow,	29.647 29.234	27½ 25¼	+015 +022	29.662 29.256	359.	+ 7. =19.7	27½ 23¼	25½	—14.5 = 5.2	353.8	– –	– –	10404
	Dec. 24. 10h 52' A.M. N.E. wind.	30.758 30.343	35 33	—010 —003	30.748 30.340	348.1	— 3.9 =11.2	34½ 30¾	32⅝	+ 1.2 = 0.4	348.5	– –	– –	10328
	1775, June 13. 11h 7' A.M. S.W. wind.	30.044 29.674	69 69	—121 —117	29.923 29.557	320.7	—31.3 =97.6	67½ 72½	70	+96. =30.8	351.5	– –	– –	11416
	1776, May 10. 10h 30' A.M.	30.096 29.706	53 51¼	—069 —063	30.027 29.643	335.4	—16.6 =50.	51½ 49¾	50¾	+46.9 =15.7	351.1	– –	– –	10971
	May 30. 11h 40' A.M. S.W. wind.	29.900 29.521	66 63	—111 —100	29.789 29.421	323.9	—28.1 =86.7	66 63	64½	+81.8 =26.5	350.4	353.5	+ 1.5	11447
	June 20. 0h 15' P.M.	30.268 29.898	71½ 71½	—129 —127	30.139 29.771	320.1	—31.9 =99.7	71 71½	71¼	+101. =32.3	352.4	– –	– –	11416
	July 16. 0h 15' P.M.	29.625 29.253	67½ 67½	—113 —112	29.512 29.141	329.6	—22.4 =68.	67¾ 65½	66¾	+87.4 =28.8	358.4	– –	– –	11355
	Aug. 26. 10h 35' A.M.	30.132 29.738	59½ 57¼	—092 —082	30.040 29.656	335.2	—16.8 =50.1	59½ 56½	57¾	+65. =21.8	357.0	– –	– –	10887
	Aug. 27. 11h 45' A.M.	30.020 29.631	62¼ 60	—099 —091	29.921 29.540	334.	—18. =54.	62 58¼	60	+70.2 =23.4	357.4	– –	– –	11028
	Sept. 2. 10h 15' AM.	29.294 28.918	60 58½	—089 —084	29.205 28.834	333.1	—18.9 =56.8	59½ 59½	59½	+69. =23.	356.1	– –	– –	11355
Pagoda in Kew-gardens, 116.5 feet.	1773, Dec. 20. 1h 22' P.M. mean of 6 obfervations with 3 barometers.	29.351 29.226	49¼ 49¼	—052 —052	29.299 29.174	111.4	— 5.1 =45.8	49¼ 49¼	49¼	+43.3 = 4.8	116.2	– –	—0.3	11184
Gun-wharf of Woolwich-warren, and upper ftory of Shooter's-hill inn, 444 feet.	1774. Apr. 27. 4h P.M. mean of 4 obf.	29.762 29.282	57¼ 56½	—082 —077	29.680 29.205	420.4	—23.6 =56.1	52½ 58½	55½	+59.2 =24.9	445.3	– –	– –	11170
	Apr. 27. 6h 30' P.M. mean of 2 obf.	29.773 29.302	54 55¼	—072 —074	29.701 29.228	418.3	—25.7 =61.4	49 49½	49¼	+43.5 =18.2	436.5	438.9	—5.1	11217
	Apr. 28. 5h A.M. mean of 5 obf.	29.805 29.336	44½ 48¾	—041 —053	29.764 29.283	424.5	—19.5 =46.	43 41¼	42⅛	+24.5 =10.4	434.9	– –	– –	11077

N° II.

N° II. Computations of barometrical obfervations made on heights near Taybridge in Perthfhire; and N° III. of thofe near Lanark.

N° II. near Taybridge.

Geometrical heights of the ftations in feet.	Date of the obfervations, winds, &c.	Obferved heights of the inferior and fuperior barometers.	Temperature of the quickfilver.	Equation for the heat of the quickfilver.	Equated heights of the barometers.	Logarithmic refult in feet.	Logar. excefs or defect in ft. and alfo in 1000th parts.	Temperature of the air, Particular.	Temperature of the air, Mean.	Equation by the rule in 1000th parts, and alfo in feet.	Refult by the rule, Particular.	Refult by the rule, Mean.	Error of the rule in feet.	Ratio of the [illegible]
Station at Weem, and top of Weem-craig, 700¼ feet.	1774, July 16. 11h 30′ A.M. bright Sun-fhine.	29.996 29.237	69½ 65¾	—122 —107	29.874 29.130	657.2	—43.2ft. = 65.3/1000	65½ 60	62¾	+ 27/1000 = 50.6ft.	707.8	— —	7.6	11249
Ditto ftation, and top of Bolfrack's cairn, 1076½ feet.	July 16. 6h 30′ P.M. calm and cloudy.	29.933 28.788	61¾ 58½	—097 —083	29.836 28.705	1007.	— 69.5 = 69.	60 56¾	58⅜	+ 67.5 = 68.5	1075 5	— —	— 1.	11382
Ditto ftation, and top of Dull-craig, 1244¼ feet.	July 18. 7h 20′ A.M.	29.825 28.500	58¼ 55	—086 —072	29.739 28.428	1174.	— 70.2 = 60.	57 55	56	+ 60. = 70.4	1244.4	— —	— —	11354
Ditto ftation, and top of Knock-farle, 1364½ feet.	July 18. 5h 4′ A.M.	29.816 28.347	55¾ 51	—077 —059	29.739 28.288	1303.5	— 61. = 46.8	54 48½	51¼	+ 47.5 = 62.	1365.5	— —	+ 1.	11254
Ditto ftation, and that in Glenmore, 1279¾ feet.	July 12. 7h 30′ P.M.	29.528 28.161	58 51¼	—084 —060	29.444 28.101	1216.5	— 62.7 = 51.6	55 51½	53¾	+ 52. = 63.2	1279.7	— —	— —	11396
Ditto ftation, and South obfervatory on Schihallien, 2098 ft.	July 11. 7h 30′ P.M.	29.643 27.432	58¾ 48	—086 —048	29.557 27.384	1989.8	—108.2 = 54.4	58 47¾	52½	+ 51. =101.5	2091.3	— —	— 6.7	11554
Ditto ftation, and Weft fummit of Schihallien, 3281 feet.	July 11. 7h 30′ A.M.	29.595 26.194	59½ 40	—089 —040	29.506 26.154	3142.3	—138.7 = 44.1	56 45	50½	+ 43.5 =136.7	3279.	— —	— 2.	11693
	July 12. 5h A.M.	29.610 26.223	50¾ 44	—062 —035	29.548 26.188	3145.5	—135.5 = 43.1	50¼ 42	46⅛	+ 34.3 =107.	3252.5	— —	—29.5	11690
Station in Glenmore, and the South obfervatory, 818.76.	July 12. 8h P.M.	28.161 27.325	51¼ 48½	—060 —050	28.101 27.275	777.4	— 41.4 = 53.2	51½ 48	49¾	+ 42. = 32.6	810.	— —	— 8.8	11851
The obfervation on Schihallien on July 11, by Mr. DE LUC's equation for the air, — — —						3142.3	50°.5—39°.7=10°.8×2.1			+ 22.7 = 71.3	3213.6	— —	—67.4	

N° III. near Lanark.

Geometrical heights of the ftations in feet.	Date of the obfervations, winds, &c.	Obferved heights of the inferior and fuperior barometers.	Temperature of the quickfilver.	Equation for the heat of the quickfilver.	Equated heights of the barometers.	Logarithmic refult in feet.	Logar. excefs or defect in ft. and alfo in 1000th parts.	Temperature of the air, Particular.	Temperature of the air, Mean.	Equation by the rule in 1000th parts, and alfo in feet.	Refult by the rule, Particular.	Refult by the rule, Mean.	Error of the rule in feet.	Ratio of the [illegible]
Level of the Clyde at Lanark Bridge, and the ftation at the garden, 362½ feet.	1774, Aug. 20. 6h 30′ A.M.	29.776 29.383	62½ 61¼	—099 —094	29.677 29.289	342.9	—19.6 =57.1	62 62	62	+ 74. = 25.4	368.3	— —	— —	11083
	Aug. 23. 3h 8′ P.M.	29 956 29.563	64¾ 65	—107 —106	29 849 29.457	344.5	—18. =52.3	63 63	63	+ 77.8 = 26.8	371.3			
	Sept. 5. 8h A.M.	29.626 29.232	52½ 50½	—067 —060	29.559 29.172	343.4	—19.1 =55.6	52½ 49½	51	+ 46. = 15.8	359.2	364.4	+ 1.9	11182
	Sept. 7. 7h 47′ A.M.	29.864 29.467	50½ 51	—061 —062	29.803 29.405	350.3	—12.2 =34.8	45 44	44½	+ 30.5 = 10.7	361 0	— —	— —	10875
	Sept. 7. 9h A.M.	29.886 29.488	50½ 51¼	—061 —063	29.825 29.425	351.8	—10.7 =30.4	47 41	44	+ 29. = 10.2	362 0			
Level of the Clyde, and Stonebyre-hill, 654 feet.	Sept. 7. 8h 15′ A.M.	29.872 29.148	48¾ 46¼	—055 —045	29.817 29.103	631.6	—22.4 =35.4	46¾ 45¾	45¼	+ 32.3 = 20.4	652.	— —	—2.0	10946
Carmichael-well, and Weft-end of Carmichael-hill, 451½ feet.	July 30. 5h 40′ P.M. S.W. wind, beginning to rain.	29.162 28.690	56 54½	—076 —071	29.086 28.619	421.8	—29.7 =70.3	54¾ 53¼	54	+ 52.3 = 22.	443.8	— —	—7.7	11430
	Aug. 1. 11h 40′ A.M.	29.612 29 135	58¾ 60	—086 —089	29.526 29.046	427.1	—24.4 =57.1	57 54¾	55⅞	+ 57.1 = 24.4	451.5	— —	— —	
Carmichael-well, and top of Tinto, four feet below the fummit of the Cairn, 1642.5 ft.	June 30. 1h 30′ P.M.	28.991 27.284	61½ 55¼	—093 —069	28.898 27.215	1563.6	—78.9 =50.5	58 51	54½	+ 53. = 83.3	1646.9	— —	— —	11684
	July 30. 6h A.M.	29 063 27.335	51½ 46¼	—062 —043	29.001 27.292	1582.6	—59.9 =38.	51 44	47½	+ 36.4 = 57.4	1640.	— —	— —	11412
	Aug. 2. 8h 15′ A.M.	29.608 27.846	54¼ 47½	—072 —048	29.536 27.798	1580.5	—62.2 =39 3	51½ 44½	48	+ 39 6 = 62.4	1642.7			
	Aug. 27. 11h 50′ A.M. S.W. wind.	28.710 27.008	59¾ 53¾	—087 —063	28.623 26.945	1570.3	—72.2 =46.	55¼ 47¼	51½	+ 45.6 = 72.3	1642.6	1645.5	+ 3.0	
	Aug. 27. 1h 40′ P.M. hail.	28.736 27.032	60¼ 53	—090 —062	28.646 26.970	1571.	—71.5 =45.5	55½ 50	52¾	+ 49. = 76.8	1647.8	— —	— —	11704
	Aug. 27. 1h 50′ P.M.	28.716 27.010	58½ 52¼	—083 —061	28.633 26.949	1579.5	—63. =40.	55½ 48¼	52	+ 46.4 = 73.3	1652.8			
Mean of the obfervations on Tinto, with Mr. DE LUC's equation for the air, — — —						1574.5	51°—39°.7=11.3×2.1			+ 24.8 = 39.	1613.5	— —	—29.	

N° IV. Computations of barometrical observations on heights near Edinburgh.

Geometrical heights of the stations in feet.	Date of the observations, winds, &c.	Observed heights of the inferior and superior barometers.	Temperature of the quicksilver.	Equation for the heat of the quicksilver.	Equated heights of the barometers.	Logarithmic result in feet.	Logar. excess or defect in ft. and also in 1000th parts.	Temperature of the air, Particular.	Temperature of the air, Mean.	Equation by the rule in 1000th parts, and also in feet.	Result by the rule, Particular.	Result by the rule, Mean.	Error of the rule in feet.	[illegible]
Leith Pier-head, and Calton-hill, 344 feet.	1774, Aug. 12. 5h 20′ A.M.	30.086 29.704	52°½ 49¾	—067 —058	30.019 29.646	325.8	—18.2ft. —$\frac{56}{1000}$	50°¾ 49	50°	+$\frac{47}{1000}$ =15.3f.	341 1	- -	- -	11037
	Aug. 15. 6h 45′ A.M.	29.568 29.197	55¼ 53½	—075 —068	29.493 29.129	323.6	—20.4 =60.3	54 54½	54¼	+54. =17.5	341.1	339.5	—4.5	
	Aug. 15. 0h 15′ P.M. S.W. wind and rain.	29.625 29.282	56¼ 53½	—078 —068	29.547 29.214	319.1	—24.9 =78.	54 54½	54¼	+54. =17.2	336.3	- -	- -	11761
Leith Pier-head, and top of Arthur's Seat, 803 feet.	Aug. 15. 5h 15′ A.M. S.W. wind and rain.	29.567 28.704	55¼ 51¾	—075 —062	29.492 28.642	762.	—41. =53.8	54 50½	52¼	+50. =38.	800.	- -	—3.	11309
Leith Pier-head, and Kirk Yetton cairn, 1544 feet.	Sept. 15. 10h 30′ A.M. S.W. wind.	29.953 28.291	57½ 52¼	—084 —063	29.869 28.228	1472.5	—71.5 =48.6	54½ 47¼	51	+47. =69.2	1541.7	- -	—2.3	11249
Calton-hill, and Kirk-Yetton cairn, 1200 feet.	Sept. 15. 1h 15′ P.M. S.W. wind.	29.561 28.272	63¾ 54	—100 —068	29.461 28.204	1136.2	—63.8 =56.2	56½ 48½	52½	+52. =59.	1195.2	- -	—4.8	11410
Level of Hawk-hill study, and bottom of Small-rock, 7.4 ft. below the top of Arthur's Seat, 702.4 feet.	1774, Dec. 1. 2h 45′ P.M.	29.565 28.770	35 32	—010 — —	29.555 28.770	701.5	— 0.9 = 1.3	33 30½	31¾	— 0 5 = 0.3	701.2	- -	- -	10724
	Dec. 10. 9h 46′ A.M.	29.494 28.687	20½ 20½	+038 +037	29.532 28.724	722.9	+20.5 =28.3	20½ 20½	20½	—26.6 =19.2	703.7	705.3	+2.9	11445
	1775, Jan. 26. 1h 35′ P.M.	29.490 28.674	26½ 24¼	+018 +026	29.508 28.700	723.5	+21.1 =29.	26 23	24½	—17. =12.4	711.1	- -	- -	10419
Base of Hawk-hill observatory, and bottom of the Small-rock on Arthur's Seat, 684 feet.	Nov. 10. 11h 30′ A.M.	29.959 29.177	38 34	—020 —006	29.939 29.171	677.2	— 6.8 =10.	36¾ 34	35½	+ 8.6 = 5.8	683.	- -	- -	10646
	Nov. 17. 9h 30′ A.M.	29.543 28.769	33¼ 30¼	—004 +005	29.539 28.774	683.8	—— ——	32 29¼	30⅝	— 3.5 = 2.4	681.4	- - 684.5	- - +0.5	10729
	1776, Jan. 31. 10h 45′ A.M.	30.009 29.229	15½ 24	+056 +026	30.065 29.225	711.7	+27.7 =39.	14 20	17	—35.2 =25.	686.7	- -	- -	10184
	July 25. 2h 20′ P.M.	30.157 29.427	70¼ 66½	—125 —111	30.032 29.316	628.8	—55.2 =87.7	69¾ 67	68½	+94. =58.2	687.0	- -	- -	11416
Hawk-hill garden-door, and bottom of the rock on Arthur's Seat, 730.8 feet.	1775, Dec. 27. 11h 30′ A.M.	29.807 28.985	30¾ 29¼	+004 +007	29.811 28.992	725.9	— 4.9 = 6.7	29½ 29½	29½	— 5.7 = 4.1	721.8	- -	- -	10707
	Dec. 27. 8h 40′ A.M.	29.778 28.945	35¾ 33	—013 —003	29.765 28.942	730.6	—— ——	35¾ 32½	34¼	+ 5.4 = 4.	734.6	- - 731.7	- - +3.9	1 642
	1776, Feb. 1. 9h 30′ A.M.	29.883 29.032	28¾ 26½	+011 +019	29.894 29.051	745.4	+14.6 =19.6	24½ 26½	25½	+15.4 =11 5	733.9	- -	- -	10390
	Aug. 3. 2h 20′ P.M.	30.135 29.348	75½ 72	—141 —127	29.994 29.221	680.4	—50.4 =74.	72¾ 69	73¾	+100. =68.	748.4	- -	- -	11286
In these two last observations Mr. DE LUC's equation for the air being substituted,	1776, Feb. 1.					745.4	39°.7—25°.5=14°.2 × 2.1			—29.8 =22.2	723.2	- -	—7.6	
	Aug. 3.					680.4	70°.7—39°.7=31°. × 2.1			+65.1 =41 3	724.7	- -	—6.1	

N° V. Computations of barometrical obfervations made on heights near Linhoufe. and N° VI. of thofe near Carnarvon in North Wales.

N° V. near Linhoufe.

Geometrical heights of the ftations in feet.	Date of the obferva-tions, winds, &c.	Obferved heights of the inferior and fuperior barometers.	Temperature of the quickfilver.	Equation for the heat of the quick-filver.	Equated heights of the barometers.	Logarithmic refult in feet.	Logar. excefs or defect in ft. and alfo in 1000th parts.	Temperature of the air, Parti-cular.	Temperature of the air, Mean.	Equation by the rule in 1000th parts, and alfo in feet.	Refult by the rule, Particu-lar.	Refult by the rule, Mean.	Error of the rule in feet.	Ratio of the weight of quick-filver to air, air being 1.
Linhoufe and Eaft-cairn hill, 5 feet below the fummit, 1176.6 feet.	1775, Nov 11. 8h A.M. calm and clear.	29.216 27.912	32° 30	—— +006	29.216 27.918	1184.2	+7.6 ft. = 6.4/1000	32° 29	30°½	+ 3.4/1000 = 4. ft.	1180.	1181.3	+ 4.7	10894
	Nov. 15. noon.	28.941 27.632	32 27	—— +015	28 941 27.647	1191.9	+ 15.3 = 12.8	32 26	29	— 7. = 8.3	1182.6			
Linhoufe, and Eaft-cairn hill, 18 feet below the top, 1165.6 feet.	1776, Dec. 17. 2h P.M.	28.990 27.688	31½ 24	+001 +025	28.991 27.713	1174.8	+ 9.2 = 7.9	30¼ 22	26⅛	— 14. = 16 4	1158.4	– –	— 7.2	10910
	Subftituting Mr. de luc's equation for the air,					—	39°.7—26°.1=13°.6×2.1			= 28 6 = 33.6	1141.2	– –	—24.4	
Linhoufe, and Weft-cairn hill, 11 ft. below the top, 1178.4 ft.	1775, Dec. 1. 1h P.M. high S.W. wind, fog above.	29.250 28.003	49 45	—055 —042	29.195 27.961	1125.3	— 53.2 = 47.3	48° 45	46°½	+ 35. = 39.4	1164.7	– –	—13.7	11441
Linhoufe, and Corfton hill, 4 feet below the top, 386.5 ft.	Dec. 8. 1h P.M. clear and windy.	29.686 29.521	41 39	—029 —023	29.657 29 288	379.7	— 6.8 = 18.	40 39	39½	+ 18. = 6.8	386.5	– –	– –	10736
Corfton hill, and Weft-cairn hill, 792 feet.	1776, Dec. 16h 11′ A.M. high N. wind, clear weather.	28.580 27.714	34¾ 32	—009 ——	28.571 27.714	793.6	+ 1.6 = 2.	34¼ 30	32⅛	+ 0.3 = 0.2	793.8	– –	+ 1.8	11077
Corfton hill, and Eaft-cairn hill, 776.6 feet.	Dec. 17. 1h A.M. light W. wind.	28.574 27.710	32 25	—— +022	28.574 27.732	779.4	+ 2.8 = 3.6	31 23½	27¼	— 10.2 = 9.3	770.1	– –	— 6.5	11068
Linhoufe, and Corfton hill, 388.5 feet.	Nov. 20. 1h P.M. fnow had fallen, high W. wind.	27.992 27.582	35 33	—009 —003	27.983 27.579	379.	— 9.5 = 28.2	33 33	33	+ 2 2 = 0 8	379.8	– –	— 8.7	11540

N° VI. near Carnarvon.

Geometrical heights of the ftations in feet.	Date of the obferva-tions, winds, &c.	Obferved heights of the inferior and fuperior barometers.	Temperature of the quickfilver.	Equation for the heat of the quick-filver.	Equated heights of the barometers.	Logarithmic refult in feet.	Logar. excefs or defect in ft. and alfo in 1000th parts.	Temperature of the air, Parti-cular.	Temperature of the air, Mean.	Equation by the rule in 1000th parts, and alfo in feet.	Refult by the rule, Particu-lar.	Refult by the rule, Mean.	Error of the rule in feet.	Ratio of the weight of quick-filver to air, air being 1.
Carnarvon Quay, and Moel Eilio, 2371 feet.	1775, Aug. 4. 1h 7′ P.M. rain above, clear below.	29.693 27.714	62½ 54	—098 —066	29.595 27.148	2248.8	—122.2 + 54.4	62½ 51	56¾	+ 59.6 =134.	2382.8	– –	– –	11594
	Aug. 8. 0h 7′ P.M. S. wind, and hazy weather above.	30.036 27.543	68 57	—118 —075	29.918 27.468	2226.3	—194.7 = 65.	68¾ 56	62⅜	+ 75. =167.	2393.3	2391.8	+20.8	
	Aug. 8. 2h 7′ P.M. S. wind, weather fomething clearer.	30.027 27.533	69½ 58¼	—122 —079	29.905 27.454	2228.3	—142.7 = 64.	69½ 57	63¼	+ 76.8 =171.	2399.3	– –	– –	11566
	Subftituting Mr. de luc's equation for the air,					2231.1	60°.8—39°.7=21°.1×2.1			+ 44.3 = 98.8	– –	2330.	—41.	
Carnarvon Quay, and Peak of Snowdon, 3555 feet.	Aug. 7. 6h 7′ A.M.	30.154 26.462	56¾ 47½	—081 —045	30.073 26.417	3377.6	—177.4 = 52.5	56⅝ 45⅛	50¾	+ 45. =152.6	3530 2	– –	– –	11646
	Aug. 7. 9h 7′ A.M.	30.165 26.468	60 49¼	—092 —050	30.073 26.418	3376.6	—178.4 = 52.8	60 47¼	53¾	+ 52.5 =177.4	3554.	3551.		
	Aug. 7. 0h 7′ P.M.	30.140 26.488	61½ 60½	—097 —083	30.043 26 405	3363.4	—191.6 = 57.	61¾ 54	57⅞	+ 61. =205.	3568.4	– –	– –	11704
	Aug. 7. 2h 7′ P.M.	30.144 26.478	62 53¾	—099 —063	30.045 26.415	3355.3	—199.7 = 59.5	62 51	56½	+ 58.5 =196.	3551.3			
	Aug. 14. 8h 7′ A.M. fog above.	29.984 26.271	56½ 42¾	—080 —031	29.904 26.240	3405.9	—149.1 = 43.8	55¼ 43	49⅛	+ 40. =136.2	3542.1	– –	– –	11643
	Aug. 14. 9h 7′ fog and rain.	29.978 26.279	58½ 44	—087 —035	29.891 26.244	3390.6	—164.4 = 48.5	57¾ 43½	50¼	+ 44.3 =150.4	3541.			
	Aug. 14. 10h 7′.	29.972 26.280	60 44½	—091 —036	29.881 26.244	3381.9	—173.1 = 51.2	60 44¾	52¼	+ 48. =162.7	3544.6	3546.8		
	Aug. 14. 11h 7′.	29.974 26.280	61½ 44¾	—097 —037	29.877 26.243	3379.4	—175.6 = 52.	61 45	53	+ 50. =169.	3548.4	– –	– –	11704
	Aug. 14. 0h 7′,	29.976 26.282	62½ 46½	—100 —042	29.876 26.240	3381.5	—173.5 = 51.3	62 46	54	+ 52.3 =176.2	3557.7			
	Barometrical height of Snowdon from the mean of two days obfervations, — —					3379.1	—175.9 = 52.1	— —	53.1	=176.3	– –	3548.9	—6.1	
	Mr. de luc's equation for the air, — —						53°.1—39°.7=13°.4×2.1=28.14			= 95.1	– –	3474.2	—80.8	

Compu-

Computations of part of Mr. DE LUC's barometrical observations, answering to the coldest and hottest temperatures of the air.

Stations with their geometrical heights in feet.	Date of the observations.	Observed heights of the inferior and superior barometers.	Temperature of the quicksilver.	Equation for the heat of the quicksilver.	Equated heights of the barometers.	Logarithmic result in feet.	Logar. excess or defect in ft. and also in 1000th parts.	Temperature of the air, Particular.	Mean	Reduced.	Equation by the rule in 1000th parts, and also in feet.	Result by the rule, Particular.	Mean.	Error of the rule in feet.	Ratio of the weight of quicksilver to air, air being 1.
Coldest of the Sun-rising Observations.															
1st. 230.5	1760, Feb. 9. $8^h\ 30'$ A.M.	28.986 28.703	$33\frac{3}{4}$ $26\frac{1}{4}$	−006 +018	28.980 28.721	233.9	+ 3.4 ft. = $\frac{14.5}{1000}$	$24\frac{3}{4}°$ $26\frac{1}{4}$	$25\frac{1}{4}$	—	− $\frac{15.4}{1000}$ = 3.6 ft.	229.9	231.8	+ 1.3	10598
	March 9. 6^h A.M.	28.875 28.586	$37\frac{1}{2}$ $28\frac{3}{4}$	−018 +012	28.857 28.598	234.9	+ 4.4 = 18.7	$27\frac{1}{2}$ 32	$29\frac{3}{4}$	—	− 4.9 = 1.2	233.7	− −	− −	10598
2d. 457.	March 9. $6^h\ 8'$ A.M.	28.875 28.342	$37\frac{1}{2}$ 30	−018 +006	28.857 28.348	463.7	+ 6.6 = 14.5	$27\frac{1}{2}$ $31\frac{1}{2}$	$29\frac{1}{2}$	—	− 5.6 = 2.6	461.1	− −	+ 4.1	10732
3d. 624.5	March 9. $6^h\ 15'$ A.M.	28.875 28.170	$37\frac{1}{2}$ 32	−018 —	28.857 28.170	627.8	+ 4.3 = 7.	$27\frac{1}{4}$ 34	$30\frac{5}{8}$	—	− 3.5 = 2.3	625.5	− −	+ 1.	10876
4th. 776.7	March 9. $6^h\ 30'$ A.M.	28.875 28.009	$37\frac{1}{2}$ 32	−018 —	28.857 28.009	777.2	+ 0.5 = 0.7	$27\frac{1}{4}$ $32\frac{1}{2}$	30	—	− 4.4 = 3.4	773.8	− −	− 2.9	10978
5th. 977.2	March 9. $6^h\ 45'$ A.M.	28.875 27.798	$37\frac{1}{2}$ $33\frac{1}{4}$	−018 −005	28.857 27.793	978.9	+ 1.7 = 1.7	$27\frac{1}{4}$ 32	$29\frac{3}{4}$	—	− 5. = 4.9	974.0	− −	− 3.2	11000
Coldest of the ordinary observations.															
2d, 457.	Feb. 9. 9^h A.M.	28.997 28.470	32 $28\frac{3}{4}$	— +012	28.997 28.482	466.9	+ 9.8 = 21.	$25\frac{1}{4}$ 29	$27\frac{1}{8}$	26°	− 14. = 6.5	460.4	− −	+ 3.4	10649
3d, 624.5	Feb. 9. $9^h\ 15'$ A.M.	28.997 28.298	32 30	— +006	28.997 28.304	630.3	+ 5.8 = 9.2	28 30	29	28	− 9. = 5.7	624.6	− −	− −	10814
4th. 776.7	Feb. 9. $9^h\ 30'$ A.M.	28.997 28.142	32 32	— —	— —	780.2	+ 3.6 = 4.5	$40\frac{1}{2}$ 32	$36\frac{1}{4}$	$30\frac{1}{2}$	− 3.5 = 2.7	777.5	− −	− −	10901
5th. 977.2	Feb. 9. 10^h A.M.	28.997 27.931	$33\frac{3}{4}$ $35\frac{1}{2}$	−006 −011	28.991 27.920	980.8	+ 3.6 = 3.5	$40\frac{3}{4}$ 37	$38\frac{7}{8}$	$31\frac{1}{2}$	− 1.1 = 1.1	979.7	− −	+ 2.5	10949
6th. 1298.9	Feb. 9. $10^h\ 15'$ A.M.	29.002 27.604	32 $37\frac{1}{2}$	— −017	29.002 27.587	1303.4	+ 4.5 = 3.5	41 $36\frac{3}{4}$	$38\frac{7}{8}$	$31\frac{1}{2}$	− 1.1 = 1.4	1302.	− −	+ 3.1	11024
7th. 1513.3	Feb. 9. $10^h\ 30'$ A.M.	29.008 27.393	$33\frac{3}{4}$ 41	−006 −027	29.002 27.366	1513.	− 0.3 = 0.2	41 $37\frac{3}{4}$	$39\frac{1}{8}$	$33\frac{1}{2}$	+ 3.3 = 5.	1518.	− −	+ 4.7	11120
8th. 1938.9	Feb. 9. 11^h A.M.	29.002 26.955	$35\frac{1}{2}$ $39\frac{1}{4}$	−011 −021	28.991 26.934	1917.7	− 21.2 = 11.0	$43\frac{1}{2}$ $37\frac{3}{4}$	$40\frac{5}{8}$	$34\frac{1}{2}$	+ 6. = 11.5	1929.2	− −	+ 9.7	11306
9th. 2094.5	Feb. 9. $11^h\ 15'$ A.M.	28.997 26.771	$35\frac{1}{2}$ $39\frac{1}{4}$	−011 −021	28.986 26.750	2091.8	− 2.7 = 1.5	$43\frac{3}{4}$ $39\frac{1}{4}$	$41\frac{1}{2}$	35	+ 6.6 = 12.6	2104.4	− −	+ 9.9	11241
10th. 2356.3	Feb. 9. $11^h\ 45'$ A.M.	28.992 26.494	$35\frac{1}{2}$ $39\frac{1}{4}$	−011 −021	28.981 26.473	2358.6	+ 2.3 = 1.	$44\frac{3}{4}$ $38\frac{3}{4}$	$41\frac{3}{4}$	35	+ 6.6 = 15.6	2374.2	− −	+ 17.9	11274
11th. 2486.3	Feb. 9. noon.	28.986 26.366	$33\frac{3}{4}$ $37\frac{1}{2}$	−006 −016	28.980 26.350	2479.	− 7.3 = 3.0	$44\frac{3}{4}$ 36	$40\frac{3}{8}$	$34\frac{1}{2}$	+ 5.6 = 14.	2493.	− −	+ 6.7	11357
Hottest of the ordinary observations on the highest stations.															
14th. 2922.	1759, July 15, 4^h P.M.	28.759 25.950	$74\frac{1}{2}$ 71	−131 −105	28.628 25.845	2664.8	−257.9 = 96.7	$88\frac{1}{2}$ 74	81	$75\frac{1}{2}$	+101. =269.2	2934.	− −	+12.	12541
15th. 3119.2	July 15. 2^h P.M.	28.797 25.778	$74\frac{1}{2}$ 71	−131 −108	28.666 25.670	2876.5	−242.7 = 84.5	85 $68\frac{3}{4}$	$76\frac{3}{4}$	$73\frac{1}{2}$	+ 96.5 =300.8	3177.3	3154.4	+35.2	12439
	July 15. $3^h\ 30'$ P.M.	28.764 25.778	$74\frac{1}{2}$ $68\frac{3}{4}$	−131 −103	28.633 25.675	2841.4	−277.8 = 97.7	$90\frac{1}{2}$ 74	82	76	+102. =290.2	3131.6	− −	− −	12603
10th. 2356.3	1760, July 20. $10^h\ 15'$ A.M.	28.775 26.499	71 $72\frac{3}{4}$	−121 −117	28.654 26.382	2152.6	−203.7 = 94.7	$87\frac{1}{4}$ 74	$80\frac{5}{8}$	75	+102. =219.6	2372.2	2371.2	+14.9	12429
	July 20. $3^h\ 15'$ P.M.	28.731 26.460	$74\frac{1}{2}$ $72\frac{3}{4}$	−131 −118	28.600 26.342	2143.	−213.3 = 99.5	$91\frac{1}{2}$ $75\frac{3}{4}$	$83\frac{5}{8}$	77	+106. =227.2	2370.2			
14th. 2486.3	July 20. $10^h\ 45'$ A.M.	28.769 26.366	71 $68\frac{3}{4}$	−121 −104	28.648 26.262	2266.	−221.3 = 97.7	$87\frac{1}{2}$ $74\frac{1}{2}$	81	75	+101.8 =230.7	2496.7	2499.2	+12.9	12468
	July 20. 3^h P.M.	28.726 26.327	$76\frac{1}{2}$ $72\frac{3}{4}$	−138 −116	28.588 26.211	2262.	−224.3 = 99.	$92\frac{1}{4}$ $76\frac{1}{2}$	$84\frac{3}{8}$	77	+106. =239.7	2501.7			
12th. 2752.	July 20. $11^h\ 30'$ A.M.	28.758 26.100	$72\frac{3}{4}$ $68\frac{3}{4}$	−126 −103	28.632 25.997	2516.3	−235.7 = 94.	88 $72\frac{1}{2}$	$80\frac{1}{4}$	74	+ 97.6 =245.6	2761.9	2769.6	+17.6	12504
	July 20. $2^h\ 30'$ P.M.	28.720 26.066	$76\frac{1}{2}$ 71	−138 −110	28.582 25.956	2511.2	−240.8 = 95.7	92 77	$84\frac{1}{2}$	77	+106. =266.2	2777.4			
13th. 2877.5	July 20. noon.	28.747 25.977	$72\frac{3}{4}$ $68\frac{3}{4}$	−126 −103	28.621 25.874	2629.3	−248.2 = 94.7	89 $73\frac{1}{2}$	$81\frac{1}{4}$	75	+101.8 =267.6	2897.1	2896.4	+18.9	12548
	July 20. $1^h\ 45'$ P.M.	28.720 25.691	$74\frac{1}{2}$ $68\frac{3}{4}$	−131 −103	28.859 25.856	2618.2	−259.3 = 99.	$92\frac{1}{4}$ 75	$83\frac{5}{8}$	77	+106. =277.5	2895.7			
					Mean of the hottest,		$\frac{95.7}{1000}$	—	81.7	75.6					

Compu-

Continuation of Mr. DE LUC's barometrical obſervations.

Stations with their geometrical heights in feet.	Date of the obſervations.	Obſerved heights of the inferior and ſuperior barometers.	Temperature of the quickſilver.	Equation for the heat of the quickſilver.	Equated heights of the barometers.	Logarithmic reſult in feet.	Logar exceſs or defect in ft. and alſo in 1000th parts.	Temperature of the air, Particular.	Mean	Reduced.	Equation by the rule in 1000th parts, and alſo in feet.	Reſult by the rule, Particular.	Mean.	Error of the rule in feet.	[illegible]
The Dole, by Sir GEORGE SHUCKBURGH's meaſurement, is above the lake of Geneva 4293 ft. Mr. DE LUC's lowermoſt barometer was higher than the lake 83 ft. hence the vertical diſtance of the barometers, 4210 feet.	1764, July 29. 1h P.M.	28.953 24.951	78¾° 67	—145 —095	28.808 24.856	3845.	—365 ft. = 94.9/1000	77¼° 64½	71¼°	70°	+ 582/1000 =339.8 f.	4184.8			
	July 29. 1h 30′ P.M.	28.942 24.940	78¾ 65¼	—145 —090	28.797 24.850	3841.	—369. = 96.	78¾ 65	71½	70	+ 88.2 =338.8	4179.8	- -	- -	12714
	1765, July 21. 10h 30′ A.M.	28.698 24.640	67 59¾	—108 —075	28.590 24.565	3954.	—256. = 64.7	67½ 51	59¼	58	+ 60. =237.	4191.	4194. - -	—16. - -	12530
	1764, July 8. 8h A.M.	28.692 24.636	71 57¾	—121 —070	28.571 24.566	3935.	—275. = 70.	73 56	64½	63	+ 72.4 =285.	4220.	- -	- -	12551
														Mean	12595
Light-houſe of Genoa, 237.6 ft	1757, June 2. 6h A.M.	30.077 29.817	76 76	—142 —141	29.935 29.686	217.7	— 20. = 92.	— —	75¾	73	+103. = 22.4	240.1			
	June 2. 4½h P.M.	30.088 29.846	82½ 84½	—163 —169	29.925 29.677	216.8	+ 21. + 96.	— —	82	79	+108. = 23.4	240.2			
	June 23. 9½h A.M.	30.116 29.857	79 75	—152 —138	29.964 29.719	214.8	+ 22.8 =107.	— —	79	76	+104. = 22.3	237.1	238.2	+ 0.6	12672
	June 23. 5h 45′ P.M.	30.041 29.796	79 79	—152 —150	29.889 29.646	212.7	— 24.9 =117.	— —	78	75	+106.5 = 22.6	235.3			
	July 26. 1h P.M.	30.021 29.774	83¾ 83¾	—166 —164	29.855 29.610	214.7	— 22.9 =107.	— —	81	77	+111. = 23.8	238.5			
								Mean	76						
For the barometrical height of Turin above Genoa,		30.019 +.071 30.090 29.197 +.069 29.266	77 77	—146 —141	29.944 29.125	722.6	— —	— —	77	—	+111.5 = 80.6	803.2			
For the barometrical height of Mr. DE LUC's room above Turin,		29.319 28.831	72¾ 72¾	—129 —126	29.190 28.705	436.6	— —	— —	72¾	—	+ 99. = 43.2	479.8			

Mr. DE LUC's room above Genoa, — — 1283.
Surface of the Lake of Geneva in ſummer below Mr. DE LUC's room, 53.3

Surface of the Lake of Geneva above the Mediterranean, — 1229.7

By Mr. DE LUC's rule the Lake is elevated above the Sea 1126 French, or 1200 Engliſh feet.

In the obſervations on the Dole, if Mr. DE LUC's equation for the air is ſubſtituted inſtead of that reſulting from the Britiſh obſervations, the barometrical height will be, } 3894 66°.6—39°.7=26°.9×2.1 {+ 56.5 =220.} =4114.—96.0 Error.

Mr. DE LA CAILLE's barometrical Obſervations, Sept. 22, 1751, at the Cape of Good Hope.

Eaſt ſignal on the Table-hill above the ſea, — 3417 Height of the obſervatory, — 14 Vertical diſtance of the barometers in feet, 3403	11h 30′ A.M.	30.174 —.173 30.001 26.502	66° 52	—111 —058	29.890 26.444	3192.	—211. = 66.	66° 50	58	—	+ 62.4 =199.2	3391.2	- -	—11.8	11713
Weſt ſignal above the ſea, — 3468 Height of the obſervatory, — 14 Vertical diſtance of the barometers, — 3454	0h 30′ P.M.	30.174 —.197 29.977 26.377	66 50	—111 —053	29.866 26.324	3289.	—179. =544.	66 50	58	—	+ 62.4 =205.2	3494.2	- -	+40.2	11662
														Mean	11687

With Mr. DE LUC's Equation for the air, } Mean 3426 — 3240 58°—39°.7=18°.3×2.1 {+ 38.4 =124.4} 3364.4 - - - - - - - - - - —62.6

Compu-

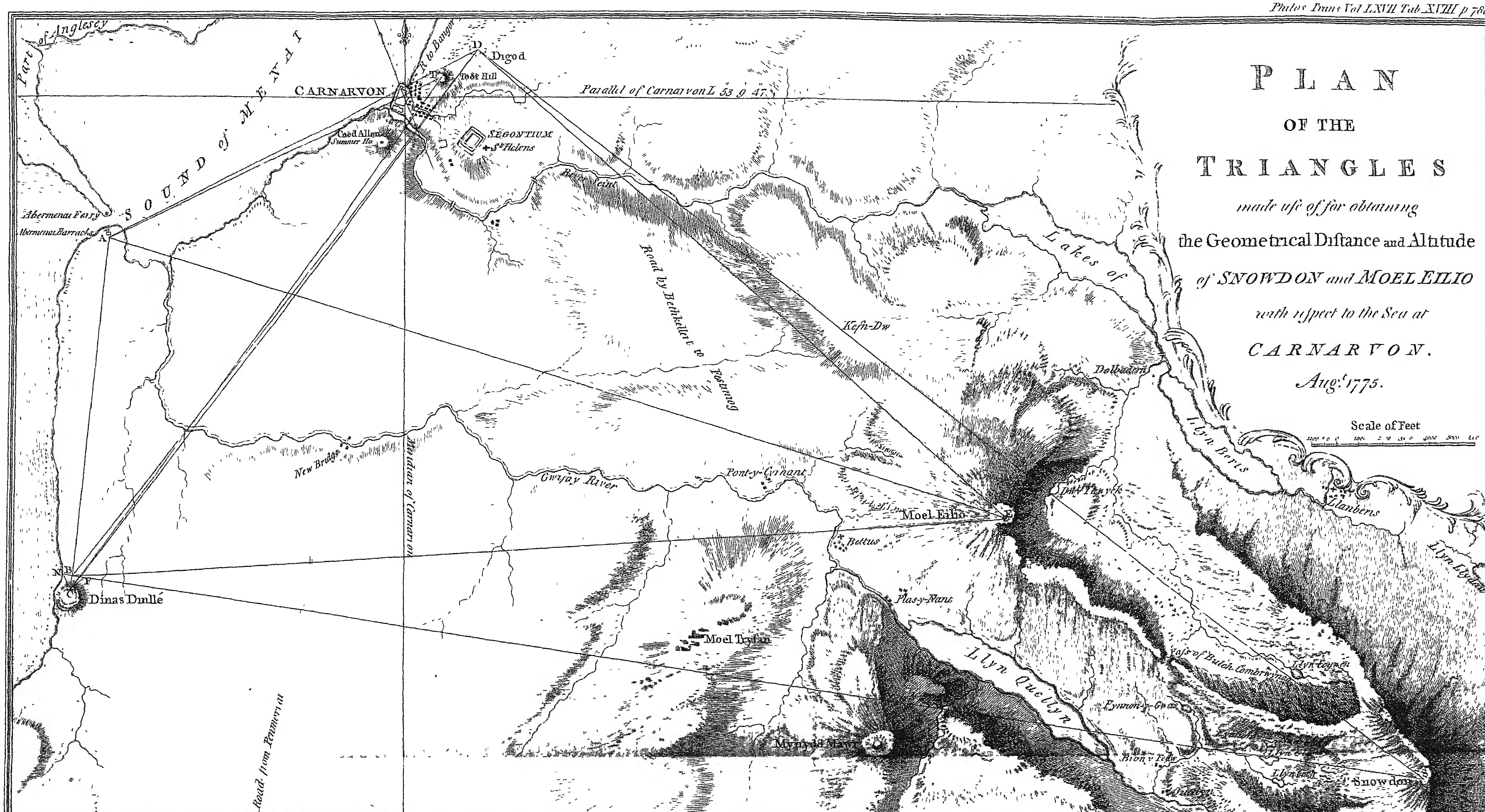
Computations of barometrical obfervations made on heights that have not been determined geometrically.

Date.	Stations of the barometers.	Obferved heights of the inferior and fuperior barometers.	Temperature of the quickfilver.	Equation for the heat of the quickfilver.	Equated heights of the barometers.	Logarithmic refult in feet.	Temperature of the air, Particular.	Temperature of the air, Mean.	Equation for the heat of the air in 1000th parts, and in feet.	Vertical diftance of the barometers.	Horizontal diftance of the barometer in miles.
	Level of the fea at Invergourie, and Belmont-caftle.	29.932 29.734	54° 57	—072 —081	29.860 29.653	181.3	54° 54	54°	+ $\frac{54.2}{1000}$ =9.8 f.	191.1	$10\frac{1}{4}$
1773, July 8.	The inferior barometer at Belmont-caftle. Superior barometer, Top of Kinpurney-hill.	29.988 28.974	65 62	—108 —095	29 880 28.879	887.9	63 57	60	+71. =63.1	951. 955.3	$2\frac{5}{8}$
1776, Sept. 12.	Ditto.	30.331 29.275	$56\frac{1}{4}$ 51	—080 —061	30.251 29.214	908.9	$57\frac{1}{2}$ 50	$53\frac{3}{4}$	+56. =50.8	959.7	$2\frac{5}{8}$
	Caftle Menzies.	29.756 29.674	$60\frac{1}{4}$ $64\frac{1}{2}$	—092 —104	29 .64 29.570	82.7	61 63	62	+74. = 6.1	88.8	$28\frac{1}{2}$
Sept. 11.	Top of Farragan.	29.794 27.344	$63\frac{3}{4}$ $52\frac{1}{2}$	—102 —062	29.692 27.282	2205.8	65 50	$57\frac{1}{2}$	+62.4 =137.8	2343.6	29
Sept. 17.	Top of Ben Lawers.	29.800 25.830	55 38	—075 —017	29.725 25.813	3677.	54 36	45	+30. =110.	3787.	42
	Top of Ben More.	30.000 26.148	$55\frac{1}{2}$ 42	—077 —029	29.923 26.119	3542.9	$52\frac{1}{2}$ 37	$44\frac{3}{4}$	+31. =109.8	3652.7	$53\frac{1}{2}$
Sept. 12.	Top of Ben Gloe.	29.712 26.142	62 48	—097 —041	29.615 26.101	3291.3	62 45	$53\frac{1}{2}$	+51. =167.9	3459.2	$27\frac{1}{4}$
Sept. 13.	Blair of Athol-lawn.	29.636 29.380	60 58	—091 —083	29.545 29.297	219.6	58 $60\frac{1}{2}$	$59\frac{1}{4}$	+67. =14.7	234.3	30
Aug. 22.	Top of King's Seat.	29.904 28.791	68 $66\frac{1}{2}$	—116 —108	29.788 28.683	985.	67 64	$65\frac{1}{2}$	+84.3 =83.	1068.	$6\frac{3}{4}$
1775, Sept. 5.	Hill of Barry.	29.870 29.345	62 56	—098 —076	29.772 29.269	444.	60 56	58	+64. =28.4	472.4	$4\frac{1}{2}$
Sept. 5.	Dunfinane-hill,	29.784 28.913	62 59	—097 —086	29.687 28.827	766.	62 59	$60\frac{1}{2}$	+71.5 =54 8	820.8	$7\frac{1}{2}$
1774, Aug. 29 and 30. mean of three obfervations.	Quay at the new bridge of Glafgow, and ftation at Lanark.	29.560 28.850	$55\frac{1}{4}$ $52\frac{3}{4}$	—077 —066	29.483 28.784	625.2	$53\frac{3}{4}$ $50\frac{3}{4}$	$52\frac{1}{4}$	+50.0 =31.3	656 5	$22\frac{1}{2}$
1772, Aug. 6. 2h P.M.	Freeport in the ifland of Ila, 19 feet above the fea, and fummit of the South-pap of Jura,	30.224 27.642	67 57	—114 —076	30.110 27.566	2300.2	60 57	$58\frac{1}{2}$	+66.3 =152.5	2452.7	$4\frac{1}{4}$
1772, Sept. 25. 9h 25' A.M.	Hafniford in Iceland, at the fea-fhore, and fummit of Mount Hecla.	29.859 24.722	49 38	—056 —016	29.803 24.706	4886.8	43 24	$33\frac{1}{2}$	+ 34 =16.6	4903 4	76

Compu-

Computations of Mr. BOUGUER's obſervations in Peru, ſuppoſing them to have been made at correſponding times, and in the mean temperature of the day, between the coldeſt of the morning and hotteſt of the afternoon.

Relative heights of the ſtations, with reſpect to the South-ſea,		Stations of the barometers, with their geometrical diſtance in feet.	Obſerved heights of the inferior and ſuperior barometers.	Temperature of the quickſilver.	Equation for the heat of the quickſilver.	Equated heights of the barometers.	Mean heights of the barometers.	Logarithmic reſult in feet.	Logar. exceſs or defect in ft. and alſo in 1000th parts.	Mean temperature of the air.	Equation for the heat of the air in 1000th parts, and in feet.	Reſult by the rule in feet.	Error of the rule.	Ratio of the weight of quickſilver to air, Particular	Mean.
Heights of the columns of air, whoſe baſes ſtood at the ſea,		15833 South-ſea, Coraçon,	29.930 16.808	$84\frac{1}{2}$° $43\frac{1}{2}$	—169 —022	29.761 16.786	23.27	14922.	— 911 ft. = $\frac{61.}{1000}$	64°	+ $\frac{61\ 6}{1000}$ =919. ft.	15841.	+ 8.0	14590	14553
		15564 South-ſea, Pichincha,	—— 16.963	$84\frac{1}{2}$ $44\frac{1}{2}$	—— —024	29.761 16.939	23.35	14685.6	—878.4 = 60.	$64\frac{1}{2}$	+ 62. =920.4	15606.	+42.	14517	
		9374 South-ſea, Quito,	—— 21.403	$84\frac{1}{2}$ $65\frac{1}{2}$	—078 ——	29.761 21.325	25.54	8685.5	—688.5 = 80.	75	+ 90. =781.7	9467.2	+93.2	13273	13120
		7840 South-ſea, Carabourou,	—— 22.625	$84\frac{1}{2}$ $66\frac{1}{2}$	—084 ——	29.761 22.541	26.15	7240.5	—599.5 = 83.	$75\frac{1}{2}$	+ 96.5 =698.7	7939.2	+99.2	12968	
Superior ſections of the columns of air, with the diſtances of their baſes from the ſea,	Coraçon, 15833 Carabourou, 7840	7993 Carabourou, Coraçon,	—— ——	$66\frac{1}{2}$ $43\frac{1}{2}$	—— ——	22.541 16.786	19.66	7681.6	—311.4 = 40.5	55	+ 35.2 =274.4	7952.	—41.	16623	16565
	Pichincha, 15564 Carabourou, 7840	7724 Carabourou, Pichincha,	—— ——	$66\frac{1}{2}$ $44\frac{1}{2}$	—— ——	22.541 16.939	19.74	7445.1	—278.9 = 37.3	$55\frac{1}{2}$	+ 36.5 =271.7	7716.8	— 7.2	16507	
	Coraçon, 15833 Quito, 9374	6459 Quito, Coraçon,	—— ——	$65\frac{1}{2}$ $43\frac{1}{2}$	—— ——	21.325 16.786	19.05	6236.5	—222.5 = 35.7	$54\frac{1}{2}$	+ 33.2 =207.	6443.5	—15.5	17149	17021
	Pichincha, 15564 Quito, 9374	6190 Quito, Pichincha,	—— ——	$65\frac{1}{2}$ $44\frac{1}{2}$	—— ——	21.325 16.939	19.13	6000.1	—189.9 = 31.6	55	+ 34. =204.	6204.1	+ 14.1	16893	
		Mean of the four ſuperior columns,	- -	- -	- -	- -	- -	- - -	$\frac{36.3}{1000}$	55	- - -	- -	- -	16793	
	Quito, 9374 Carabourou, 7840	1534 Carabourou, Quito,	—— ——	$66\frac{1}{2}$ $65\frac{1}{2}$	—— ——	22.541 21.325	21.93	1445.	— 89. = 61.6	66	+ 61. = 88.	1533.	— 1.	15089	



GEOMETRICAL OPERATIONS.

The Base AB, situated on a perfectly level Plain, was measured twice in contrary directions, between the Barracks of Abermenai and the bottom of Dinas Dinlle, an ancient British Fortification, on the Sea Shore. The length of the Iron Chain made use of on this occasion, was ascertain'd by means of accurate Deal Rods, applied to it every Morning & Evening, before and after the operation of the Field. The two measurements agreed to within less than a Foot, and made the length of the Base = 24076F. This Base AB, was afterwards prolonged to C, the Top of the Dinas by means of the side Base BF 411 7 feet. Thus the distance BC, being 482.2 feet; the total Base AC amounts to 24558 2 feet.

N, High Watermark Neap Tide. D, a small Eminence called the Digod. T, the Toot Hill of Carnarvon. S, The Peak of Snowdon. E, A Cairn of Stones on Moel Eilio.

The Latitude of Carnarvon was found to be 53° 9′ 47″, and the Variation of the Needle, by two Azimuths of the Sun, taken on the 13th of Aug.t at 4 5 and 5 12 P.M. 21.18 30 westerly.

The Height of Moel Eilio above Carnarvon Quay 2371 Feet, resulting from the simplest operation on the Digod, the nearest point to the Hill, is to be preferred to that deduced from the Angles of Elevation taken from A.

Triangles.		Angles	° ′ ″	Sides (Feet)
CBF and CBN for the prolongation of the Base BC, and distance of N from C.	Obs.d	CBF	84 10 40	BC. 482.2
		BCF	42 55 25	
	Hence	BFC	52 53 55	
	Obs.d	NBC	98 46 50	
		BCN	30 8 10	NC.... 612 5
		CNB	51 4 50	
ACD and DBC, for the distance of D from AB and C; and for the Angles CBD and BDC.	Obs.d	DAC	123.17.50	AD..17169.7
		ADC	25.48 30	CD..27949 7
		ACD	30 53 40	
	Cont.d	DCB	30.53 40	BC....482.2
		CBD	148 35 24	BD..27537 4
		BDC	0.30 56	
DSB for the distance of Snowdon from B and D.	Obs.d	ABS	92 36 15	
	−	ABD	31.24 36	
	=	DBS	61. 11 39	BS..57355.2
	Obs.d	SDC	89 35 50	DS..50258.3
	+	CDB	0.30.56	
	=	SDB	90. 6. 46	
	Hence	DSB	28 41 35	

Triangles		Angles	° ′ ″	Sides (Feet)
DEB and DAE, For the distance of Moel Eilio from D and A.	Obs.d	EBA	79.49 10	
	−	ABD	31 24 36	
	=	DBE	48.24 34	
	Obs.d	EDC	86.27.5	DE..29319 6
	+	CDB	0.30.56	
	=	EDB	86.58.1	
		DEB	44.37.25	
	Cont.d	ADE	111.15.35	
		DAE	43.40.10½	AE..39190 2
		DEA	25.55 14½	
CAT, for the distance of the Toot Hill from A.	Obs.d	CAT	122.9 17	AT..15430
		ACT	29.56.45	
		ATC	27.53.58	
Angles of Elevation and Depression	From B	..C	11.24.37	
		..S	3 29.7	
	From C Dep	..N	10.24.23	
	From D	..S	3.47.54	
		..E	4.15.35	
	From A	..E	3.24.17	
		..T	0.39.47	

Relative Heights		Feet
For the Altitude of Snowdon above B and N	S above B Trigonometrically	3493 2
	Curvature & Refraction	62.3
	Height of the Instrument	4 6
	S above B	3560.1
	C above B Trig.l ... 97 3	
	Instrument ... 4 6	
	C above B ... 101 9	
	C above N Trig.l ... 114.3	
	Instrument ... 4.6	
	C above N ... 109.7	
	Hence B above N	7 8
	Snowdon above the Sea at N	3567.9
For the Altitude of Snowdon above D and the Sea at Carnarvon	S above D Trig.l	3336.7
	Curvature & Refraction	47.8
	Instrument	4 6
	S above D	3389.1
	D above North Angle of Carnarvon Quay by Levell.g	166.3
	Vertical distance of the Barometers	3555.4
	Quay above High Water N Tide	13
	Snowdon above the Sea	3568.4

Relative Heights		Feet
For the Altitude of Moel Eilio above D, and the Sea at Carnarvon	E above D Trig.l	2183 8
	Curvature & Refraction	16 3
	Instrument	4 6
	E above D	2204.7
	D above Carnarvon Quay	166.3
	Vertical distance of Barom.rs	2371.0
	Quay above Neap Tide	13.
	Moel Eilio above the Sea	2384
For the Altitude of Moel Eilio above A, and the Sea at Carnarvon	E above A Trig.l	2331.6
	Curvature & Refraction	29.1
	Instrument	4.6
	Moel Eilio above A	2365.3
	T above A Trig.l	178 6
	Curvature & Refraction	4 5
	Instrument	4 6
	T above A	187.7
	T above Carnarvon Quay by Levelling	186 5
	Quay above A	1 2
	Hence the Vertical distance of the Barometers	2364 2
	Moel Eilio above Neap Tide	2377.1

N.B. The time not admitting of any actual Survey of the Environs of Carnarvon & Snowdon to be made, the Plan is only to be consider'd as a slight Sketch intended merely to convey a general Idea of the nature of the Country where the Triangles were situated.

Basire Sculp.t

made variable. I added that the ſame effect might be produced without the double refraction of the rock cryſtal, with a priſm made of common glaſs, only ſmaller than the aperture of the object-glaſs. The rays which paſs through the priſm would then form an image which would be ſeen out of its natural place; and thoſe which paſs without would give another image, in the ſame place it would have appeared in, if the priſm had not been there.

A few days after, the Abbé FONTANA was told, that the Abbé ROCHON had thought of another micrometer with a priſm of rock cryſtal, which, approaching more or leſs to the focus of the object-glaſs, had the advantage of producing a very great effect, and might be achromatic; that having accordingly had his inſtrument executed, and made ſeveral obſervations with it, he had prepared a paper on the ſubject, to be read at the next meeting

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ajouté que l'on pouroit avoir le même effet ſans la double réfraction du criſtal de roche, en faiſant un priſme à verre ſimple, mais plus petit que l'ouverture de l'objectif. Les rayons qui paſſent par le priſme, formeroient alors une image déplacée de ſa poſition naturelle, et les autres qui paſſeroient dehors, donneroient l'autre image, à la même place qu'elle auroit, s'il n'y avoit point de priſme.

Quelques jours après on a annoncé à M. FONTANA que M. l'Abbé ROCHON avoit imaginé un autre micrometre à priſme de criſtal de roche, qui, s'approchant plus ou moins du foyer de l'objectif, avoit l'avantage de produire un effet très grand, et de pouvoir être acromatique: qu'ayant fait executer ſon inſtrument, et fait avec lui pluſieurs obſervations, il avoit préparé un mémoire ſur

meeting of the Academy. This the Abbé FONTANA immediately told me, and I repeated what I had ſaid to him the firſt time on the effect of this micrometer; adding at the ſame time the preciſe meaſure of the ſcale for the meaſure of the ſame effect, and the facility of obtaining the ſame thing without the rock cryſtal; mentioning likewiſe other advantages which might be derived from the common glaſs priſm not covering the whole aperture of the object-glaſs; and, amongſt others, that of being able to meaſure much greater angles by this means than by the double refraction of the rock cryſtal.

The Abbé ROCHON did accordingly read his paper to the Academy, and mention has been made of it in the public prints: he has therefore the merit of having thought of the ſame thing, at the ſame time with, or perhaps before

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ſur cet objet, pour lire à la prochaine ſéance de l'Academie. Mr. l'Abbé FONTANA eut la bonté de m'en avertir immédiatement; ce fut pour lors que je lui répétai ce que j'avois eu l'honneur de lui dire la premiere fois ſur l'effet de ce micrometre, en y ajoutant la meſure préciſe de l'échelle pour la meſure du même effet, et la facilité d'obtenir la même choſe ſans le criſtal de roche, avec d'autres avantages que l'on pouvoit tirer du priſme à verre ſimple, ne couvrant pas toute l'ouverture de l'objectif, et entre autres celui de pouvoir meſurer par ce moyen des angles beaucoup plus grands, que par la double réfraction du criſtal de roche.

M. l'Abbé ROCHON a réellement lu à l'Academie ſon mémoire, et on en a fait mention dans les gazettes: ainſi il a le merite d'avoir imaginé la même choſe, dans

before me, without any knowledge whatever of my ideas on the ſubject; he has been the firſt who announced it to the world, who had it executed, and who made uſe of it: I have therefore no pretenſions whatſoever on that head; he has the merit of a great diſcovery, and aſtronomy has the ſole obligation of it to him.

But the Abbé ROCHON has only made uſe of the double refraction of the rock cryſtal for his micrometer, and I am aſſured he has ſaid, that his priſm could give him no more than ſix degrees. Now it is well known, that pieces of rock cryſtal, large enough and pure enough for theſe purpoſes, are extremely rare; beſides, the difficulty of working them is great, that ſubſtance being harder than glaſs, and requiring the utmoſt attention in cutting, in order to obtain the difference deſired between the two refractions. I think therefore, that it will be doing an eſſential

dans le même tems, peut être avant moi, et abſolument ſans avoir eu aucune connoiſſance de mes idées ſur le même objet; de l'avoir annoncé le premier au public, de l'avoir éxécute, et de s'en être ſervi le premier: ainſi je n'ai rien à prétendre de ce côté là: il a le merite d'une belle découverte, et l'aſtronomie lui en a toute l'obligation.

Mais M. l'Abbé ROCHON n'a employé, pour ſon micrometre, que la double refraction du criſtal de roche; et on m'a aſſuré qu'il a dit, que ſon priſme ne pouvoit lui donner que juſqu'à ſix degrés. On ſait bien que les pieces aſſéz grandes de cette matiere, et aſſéz pures, ſont très rares; outre la difficulté de la travailler, etant plus dure que le verre, et quelle attention il faut avoir pour la bien couper afin d'avoir la difference des deux refractions que l'on veut. Ainſi je

essential service, to propose another micrometer of common glass, to explain the theory of it, and to extend it to much larger angles, which may render it applicable to the optical instruments made use of in the navy, in taking geographical latitudes and longitudes.

I had already made a prism of this sort, and shewed the Abbé FONTANA its effect for the double image of the Sun on his excellent little achromatic glass: the two images were procured by applying this prism to the object-glass with the hand, in such a manner that it covered only one-half of it: pushing it more or less forward, occasioned a change in the brightness of the light of the two images, and shewed that they might be made equally clear. By changing the inclination of this piece, the distance between the two images was varied, which did not alter when

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je crois rendre un service encore plus considerable, en proposant cette autre espece de micrometre à verre simple, en developpant sa théorie, en l'etendant aux angles beaucoup plus grands, ce qui donne le moyen de l'appliquer aussi aux instrumens d'optique, que la marine doit employer pour observer les latitudes et longitudes géographiques.

J'avois déja fait faire un prisme de cette espece; et fis voir au même M. l'Abbé FONTANA son effet pour la double image du Soleil, sur son excellente petite lunette acromatique: on avoit les deux images, en l'appliquant à la main sur l'objectif, de maniere qu'il n'en couvroit que la moitié. En le poussant plus ou moins avant, on changeoit la vivacité de la lumiere des deux images, où l'on voyoit qu'on pouvoit les réduire à une clarté egale; en variant l'inclinaison de cette piece on varioit la distance des deux images, qui n'avoit aucune variation

when its diſtance from the object-glaſs was varied without the glaſs. This piece was a common priſm, which gave a refraction a little greater than the apparent diameter of the Sun: I added another to it afterwards, of the ſame kind and equal, both of them having circular baſes. Turning one of the two parts upon its axis, will vary the angle from o, to double each in particular, which occaſions the two images to approach to, or recede from, each other. A much ſlower variation is obtained by the greater or leſs diſtance of the priſm from the object-glaſs; but there is a particular reaſon for which one cannot give it too large a one, the contraction of the pencil of rays belonging to each point of the object, not allowing that diſtance to be very great, for fear of weakening too much the direct image towards the middle of the field, by the inter-

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tion en changeant, hors de la lunette, ſa diſtance à l'objectif. Cette piece etoit un ſeul priſme ſimple, qui donnoit une refraction un peu plus grande que le diametre apparent du Soleil: j'y ai fait ajouter après un autre ſemblable et egal; l'un et l'autre ayant les baſes circulaires: en tournant ſur ſon axe l'une des deux parties, on variera l'angle depuis zero, juſqu'au double de chacun en particulier, ce qui fait approcher et eloigner les deux images entre elles: on obtient une variation beaucoup plus lente par l'eloignement plus grand ou plus petit du priſme à l'objectif; mais il y'a une raiſon particuliere pour laquelle on ne peut pas lui en donner un trop grand; car le retreciſſement du pinceau de rayons appartenans à chaque point de l'objet, ne permet pas de l'en eloigner trop, ce qui, vers le milieu du champ, affoibliroit trop l'image directe, en interceptant une trop grande

interception of too great a part of the ſame pencil, which in the end would occaſion its being altogether loſt.

I have in hand, making for me, a rude machine in which one of the pieces may be turned by the hand upon its axis, to make the diſtance between the two images ſomewhat larger than that which is intended to be meaſured, as for inſtance the diameter of the Sun; and by the help of a moveable ſcrew, one may carry the priſm, thus compoſed, to a diſtance from the object-glaſs, by a motion ſimilar to that of the ſmall mirror of the teleſcope. I have had it adapted to an ordinary glaſs of about four feet, where its effect, for the Sun's diameter, muſt be much greater than an inch of motion in a minute; for the other planets one may have ten or fifteen lines in a ſecond, or even more. Generally the ſcale is the whole length of the glaſs for the total refraction of the priſm, which likewiſe

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grande partie du même pinceau, et à la fin la feroit perdre totalement.

Je fais faire actuellement une machine groſſiere, dans laquelle on peut tourner une des deux pieces à la main, ſur ſon axe, pour rendre la diſtance des deux images un peu plus grande que celle que l'on veut meſurer, comme du diametre du Soleil; et à l'aide d'une vis de rapel, on peut eloigner le priſme, ainſi composé, de l'objectif, par un mouvement ſemblable à celui du petit miroir des teleſcopes. Je l'ai fait adapter à une lunette ordinaire de près de quatre piés, où ſon effet, pour le diametre du Soleil, doit être de beaucoup plus d'un pouce de mouvement par minute; et pour les autres planetes on peut avoir 10 ou 15 lignes par ſeconde, et plus encore. Généralement l'échelle eſt toute la longuer de la lunette pour la

wiſe is the caſe in the Abbé ROCHON's priſm, for the difference of the two refractions. But one may vary the angle by applying the priſm without the glaſs near the object-glaſs, and turning one of the parts upon its axis. In that caſe, the ſcale of the exceſs of the ſum of the refractions of the two parts of the priſm above the difference, will be in length no more than half the circumference of a circle, though the circle may be made as large as one pleaſes; but the difference of the diſtance of the images, will not be proportional to the difference of the arcs run through by the index. In order to determine the relation which the motion of the index bears to the variation of the diſtance between the two images, one muſt have the ſolution of a geometrical problem, which is eaſily gained by ſpherical trigonomety; but it will be always better to deter-

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la refraction totale du priſme, ce qui eſt le même pour la difference des deux réfractions dans le priſme de M. l'Abbé ROCHON. Mais on peut varier l'angle en appliquant le priſme hors de la lunette, à côte de l'objectif, en faiſant tourner une des deux parties ſur ſon axe. Alors l'échelle de l'excès que la ſomme des réfractions des deux parties du priſme a ſur la difference, n'aura pour ſa longueur que la demi circonference d'un cercle, quoiqu'on puiſſe faire ce cercle auſſi grand que l'on veut; mais la difference de la diſtance des images ne ſera pas proportionelle à la difference des arcs parcourus par l'index. Pour déterminer la rélation du mouvement de l'index avec la variation de la diſtance des deux images, il faut réſoudre un probleme de géométrie, et j'en ai la ſolution bien ſimple par la trigonométrie ſphérique; mais il vaudra toujours beaucoup mieux déterminer ce

determine this relation by an actual terreſtrial obſervation of a divided ruler, obſerved at a given diſtance.

When the angle happens to be a large one, the colours would naturally be ſuch as would greatly deform one of the two images of the object, namely, that given by the rays that paſs through the priſm; but this is eaſily remedied, at leaſt in a great meaſure, by making each priſm of two pieces, one of common and the other of flint-glaſs. One may multiply the compoſed achromatic priſms with variable angles by making the one give degrees from 5 to 5, or from 2 to 2, and the other the minutes. One may put two on the outſide near the object-glaſs, which will change the diſtance of the images by the circular motion, and give the angle required a little larger than the real one; and another within which will

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ce raport par une obſervation actuelle terreſtre d'une regle diviſée, et obſervée à une diſtance donnée.

Quand il s'agit d'un grand angle, on auroit des couleurs qui déformeroient beaucoup une des deux images de l'objet, c'eſt à dire celle qui eſt donnée par les rayons paſſés à travers du priſme: on les evite aiſément, au moins en grande partie, en compoſant chaque priſme de deux pieces, une de verre commun, et l'autre de *flint-glaſs*. On peut multiplier les priſmes compoſés acromatiques et à angles variables, en faiſant que l'un donne les degrés de 5 en 5, ou de 2 en 2, et l'autre les minutes: on peut en mettre deux dehors, près de l'objectif, qui changeront la diſtance des images par le mouvement circulaire, et donneront l'angle cherché un peu plus grand que le véritable; et un autre dedans, qui donnera

will exactly give the ſeconds. I have already thought of the inſtruments requiſite for obtaining all theſe objects with accuracy, as well as for the application of a variable priſm to the common ſailor's octant, and have by me the ſolution of the neceſſary problems: this will be the object of a work I am preparing. In the mean time I publiſh this, to give others an opportunity of hitting on ſomething better concerning the mechanical conſtruction of theſe inſtruments.

donnera, avec toute préciſion, les ſecondes. J'ai déja imaginé les inſtrumens néceſſaires pour avoir avec exactitude tous ces objets, comme auſſi pour l'application d'un priſme variable à l'octant de marine ordinaire, ayant auſſi la ſolution des problemes neceſſaires. Tout cela ſera l'objet d'un ouvrage que je prépare ſur cette matiere. En attendant je publierai dans les differens journaux ce Proſpectus, pour donner plutôt à tout le monde le moyen d'imaginer ſur la forme mechanique des inſtrumens, quelque choſe de mieux que ce qui m'eſt venu dans l'eſprit ſur ce ſujet nouveau, et bien intereſſant.

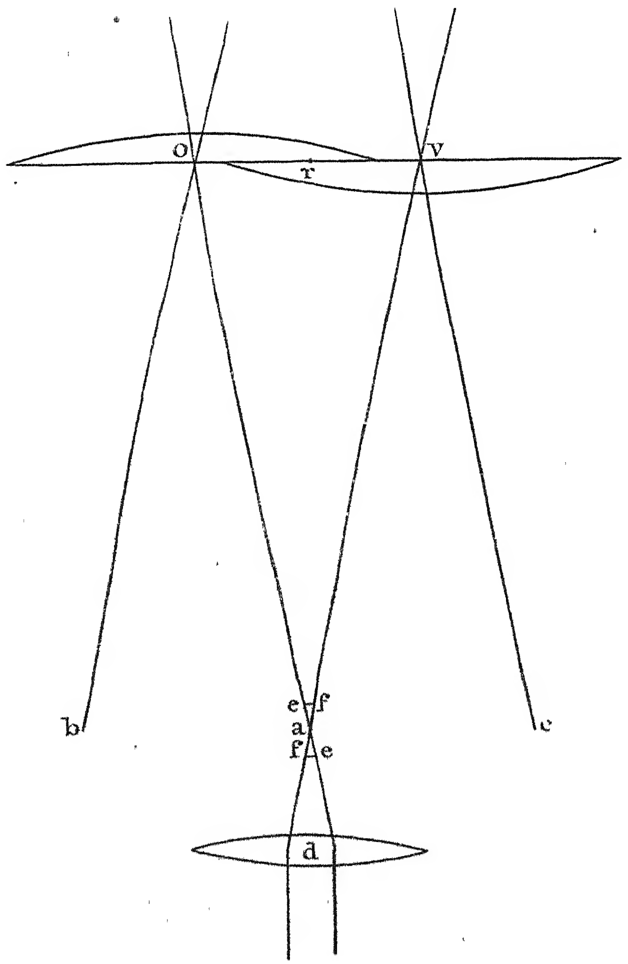

Fig: I.

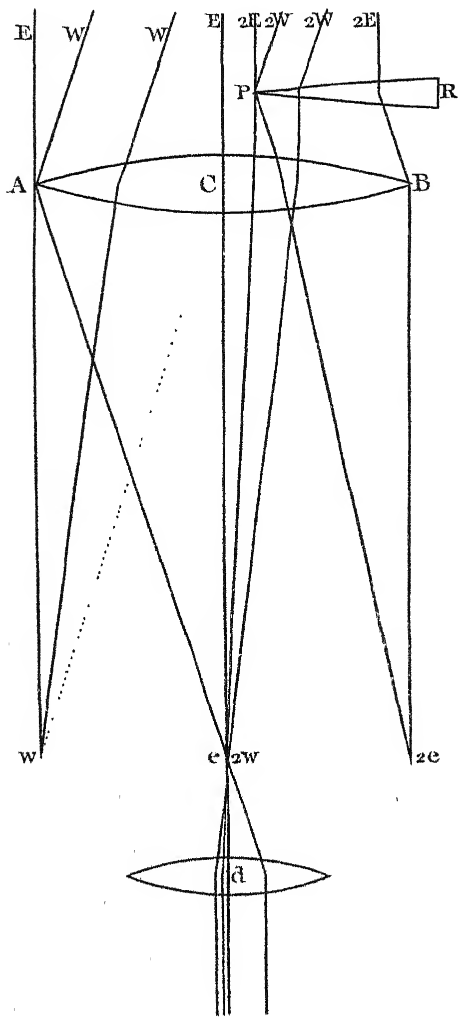

Fig: II.

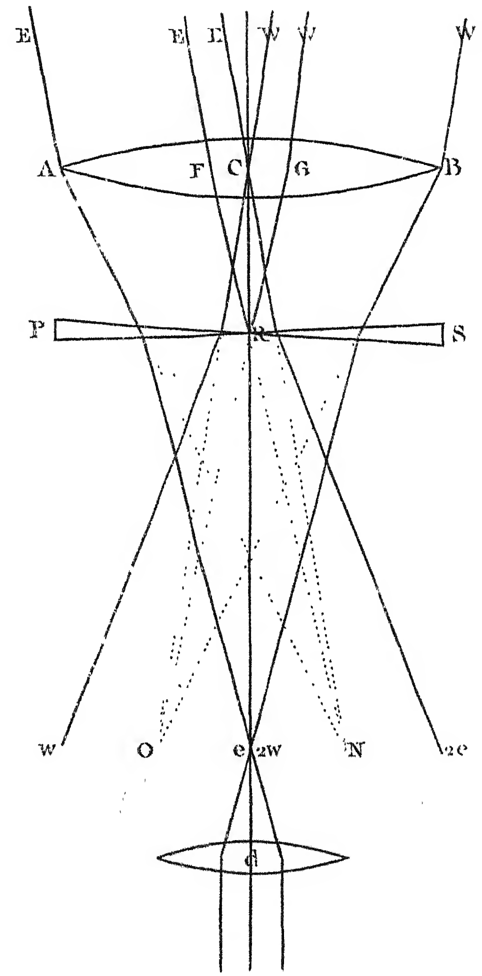

Fig: III.

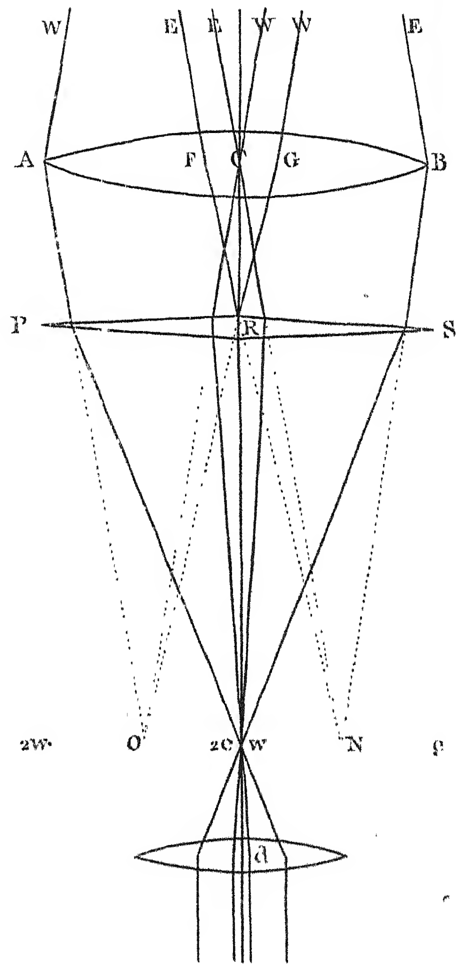

Fig: IV.

Basire sc.

XXXVI. *Account of a new Inſtrument for meaſuring ſmall Angles, called the priſmatic Micrometer.* By the Rev. Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer-Royal.*

Read Dec. 18, 1777.

PRACTICAL aſtronomy was much benefited by the invention of the wire micrometer, for meaſuring differences of right aſcenſion and declination: nor did it receive leſs advantage from Mr. SAVERY's moſt ingenious invention of the divided object-glaſs micrometer, which has been rendered more commodious by the late Mr. JOHN DOLLOND's application of it to the object-end of a reflecting teleſcope, or the preſent Mr. PETER DOLLOND's application of it to the object-end of an achromatic refracting one.

But, valuable as the object-glaſs micrometer undoubtedly is, ſome difficulties have been found in the uſe of it, owing to the alterations in the focus of the eye, which are apt to cauſe it to give different meaſures of the ſame angle at different times. For inſtance, in meaſuring the Sun's diameter, the axes of the pencils of rays, which come through the two ſegments of the object-glaſs from

contrary limbs of the Sun, croffing one another at the focus of the telefcope under an angle equal to that of the Sun's diameter, the union of the limbs of the two images of the Sun cannot appear perfect unlefs the eye be difpofed to fee objects diftinctly which are placed at point of interfection. But if the eye be difpofed to fee objects diftinctly, which are placed nearer the object-glafs than the interfection is, the two limbs will appear feparated by the interval of the axes of the pencils in that place; and if the eye be difpofed to fee objects diftinctly, which are placed farther from the object-glafs than the interfection is, the two limbs will appear to encroach upon each other by the diftance of the axes of the pencils, after their croffing, taken at that place.

To explain this, let OV (plate XIX. fig. 1.) reprefent the centres of the two femi-circular glaffes of the object-glafs micrometer, feparated to the diftance OV from each other, fubtending the angle O*a*V, equal to the Sun's diameter, at the point *a*, which is the common focus of the two pencils of rays having O*a* and V*a* for their axes, namely, thofe proceeding from contrary fides of the Sun, and paffing through the contrary femi-circles; and let *d* be the eye-glafs. It is evident, that if *d* be properly placed to give diftinct vifion of objects placed at the point *a*, the rays O*a*, V*a*, as well as all the other rays belonging

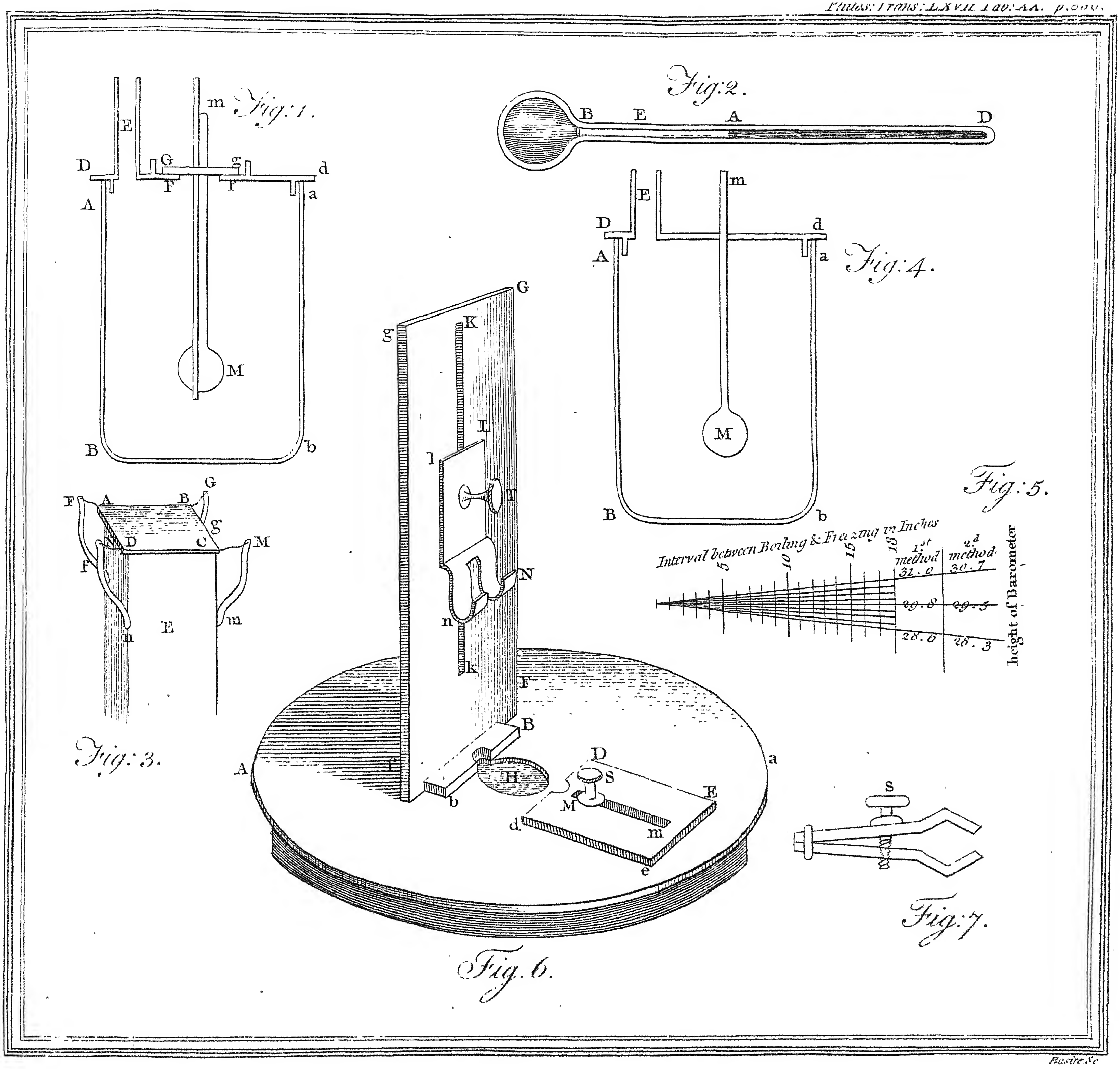
Fig: 1.
Fig: 2.
Fig: 3.
Fig: 4.
Fig: 5.
Interval between Boiling & Freezing in Inches
5
10
15
18
1st method
2d method
31.0
30.7
29.8
29.5
28.6
28.3
height of Barometer
Fig. 6.
Fig: 7.

Basire Sc.

to those pencils, will be collected into one point upon the retina of the eye; and consequently, the two opposite limbs of the two images of the Sun will seem to coincide, and the two images of the Sun to touch one another externally. But if the state of the eye should alter, the place of the eye-glass remaining the same, the eye will be no longer disposed to see the image formed at the point *a* distinctly, but to see an object placed at *ef*, nearer to or farther from the object-glass distinctly; and therefore an image will be formed on the retina exactly similar to the somewhat confused image formed by the rays on a plane perpendicular to their course at *ef*. Consequently, as the two cones of solar rays, *b* o *a*, *c* v *a*, formed by the two semi-circles, are separated or encroach upon one another at this point of the axis by the distance *ef*, the two images of the Sun will not seem to touch one another externally, but to separate or to encroach upon one another by the interval *ef*. The error hereby introduced into the measure of the Sun's diameter will be the angle *erf*, subtended by *ef* at *r* the middle point between o and v, which is to *eaf* or o*a*v, the Sun's apparent diameter, as *ae* to *cr*, or even to *ar*, on account of the smallness of *ae* with respect to *ar*.

These considerations concerning the cause of a principal error that has been found in the object-glass micro-

 meter,

meter led me to inquire, whether ſome method might not be found of producing two diſtinct repreſentations of the Sun, or any other object, which ſhould have the axes of the pencils of rays, by which they are formed, diverging from one and the ſame point, or nearly ſo: and it occurred to me, that this might be done by the refraction of a priſm placed to receive part of the rays proceeding from the object, either before or after their refraction through the object-glaſs of a teleſcope. If the priſm be placed without the object-glaſs, the rays that are refracted through it will make an angle with the rays that paſs beſide it equal to the refraction of the priſm; and this angle will not be altered by the refraction of the object-glaſs afterwards. Conſequently, two images of an object will be repreſented, and the priſm ſo applied will enable us to meaſure the apparent diameter of any object, or any other angular diſtance which is equal to the refraction of the priſm. But if the priſm be placed within the object-glaſs, that is to ſay, between the object-glaſs and eye-glaſs, the angle meaſured by the inſtrument will vary according to the diſtance of the priſm from the focus of the object-glaſs, bearing the ſame ratio to the refraction of the priſm, as the diſtance of the priſm from the focus bears to the focal length of the object-glaſs.

Let

Let ACB (fig. 2.) represent the object-glass, and *d* the eye-glass of a telescope, and PR a prism placed to intercept part of the rays coming from an object, suppose the Sun, before they fall on the object-glass. The rays EE proceeding from the Eastern limb of the Sun, and refracted through the object-glass ACB without passing through the prism, will form the corresponding point of the Sun's image at *e*; and the rays WW proceeding in like manner from the Western limb of the Sun will be refracted to form the correspondent point of the Sun's image at *w*. But the rays 2F, 2E, 2W, 2W, proceeding in like manner from the Eastern and Western limbs of the Sun, and falling on the prism PR, and thence refracted to the object-glass ACB, will, after refraction through it, form the correspondent points of the Sun's image at *2e*, *2w*. Let the refraction of the prism be equal to the Sun's apparent diameter: in this case, at whatever distance the prism be placed beyond the object-glass, the two images of the Sun *we*, *2w2e*, will touch one another externally at the point *e2w*; for the rays 2W, 2W, proceeding from the Western limb of the Sun being inclined to the rays EE proceeding from the Eastern limb in the angle of the Sun's apparent diameter, will, after suffering a refraction in passing through the prism equal to the Sun's apparent diameter, emerge

 from

from the prifm and fall upon the object-glafs parallel to the rays EE, and confequently will have their focus $2w$ coincident with the focus e of the rays EE, and therefore the two images of the Sun we, $2w2e$, will touch one another externally at the point $e2w$, and the inftrument will meafure the angle EC2W, and that only.

But if the prifm be placed within the telefcope, the angle meafured by the inftrument will be to the refraction of the prifm as the diftance of the prifm from the focus of the object-glafs is to the focal diftance of the object-glafs: or if two prifms be ufed to form the two images, with their refracting angles placed contrary ways, as reprefented in fig. 3. and 4. the angle meafured will be to the fum of the refractions of the prifms as the diftance of the prifms from the focus of the object-glafs is to the focal diftance of the object-glafs. For let ACB (fig. 3.) reprefent the object-glafs, and d the eye-glafs of a telefcope, and PR, RS, two prifms interpofed between them, with their refracting angles turned contrary ways, and the common fections of their refracting planes touching one another at R. The rays proceeding from an object, fuppofe the Sun, will be difpofed, by the refraction of the object-glafs, to form an image of the Sun at the focus; but part of them falling on one prifm, and part on the other, will be thereby refracted contrary ways, fo as to form two equal images we, $2w2e$, which,

if

if the refractions of the prifms be of proper quantities, will touch one another externally at the point *e2w*. Let ECN be the axis of the pencil of rays EE proceeding from the Sun's Eaftern limb; and WCO the axis of the pencil of rays WW proceeding from the Sun's Weftern limb; and the point N the place where the image of the Sun's Eaftern limb would be formed, and the point O where that of the Weftern limb would be formed, were not the rays diverted from their courfe by the refractions of the prifms. But by this means part of the rays EE, which were proceeding to N, falling on the prifm PR, will be refracted to form an image of the Sun's Eaftern limb at *e*, while others of the rays EE, which fall on the prifm RS, will be refracted to form an image of the Sun's Eaftern limb at *2e*. In like manner, part of the rays WW, which were proceeding to form an image of the Sun's Weftern limb at O, falling on the prifm RS, will be refracted to form an image of the Sun's Weftern limb at *2w* coincident with *e*, the point of the image correfpondent to the Sun's Eaftern limb; while others of the rays WW, which fall on the prifm PR, will be refracted to form the image of the Sun's Weftern limb at *w*. The two images *we*, *2w2e*, are fuppofed to touch one another externally at the point *e2w*. The ray EFR, which belongs to the axis ECN, and is refracted by the prifm PR to *e*, undergoes the refraction NR*e*, which (be-

caufe

cause small angles are proportional to their sines, and the sine of NR*e* is equal to the sine of its supplement NRC) is to NCR as NC or C*e* is to NR or R*e*. In like manner, the ray WGR, which belongs to the axis WCO, and is refracted by the prism RS to 2*w* or *e*, undergoes the refraction OR*e*, which is to OC*e* as OC or C*e* is to RO or R*e*; therefore, by composition, ORN the sum of the refractions OR*e*, NR*e*, is to OCN the sum of the angles OC*e*, NC*e*, or the Sun's apparent diameter, as C*e* to R*e*; that is, as the focal distance of the object-glass to the distance of the prisms from the focus of the object-glass.

Or let the prisms PR, RS, be placed with their refracting angles P, S, turned from one another as in fig. 4.: the refraction of the prism PR will transfer the image of the Sun from ON to *we*, and the refraction of the prism RS will transfer the image ON to 2*w*2*e*, the two images 2*w*2*e*, *we*, touching one another externally at the point 2*ew*. Let ECN, WCO, be the axes of the pencils of rays proceeding from the two extreme limbs of the Sun, and N, O, the points where the images of the Sun's Eastern and Western limbs would be formed by the object-glass, were it not for the refraction of the prisms; the ray EFR, which belongs to the axis ECN, and is refracted by the prism RS to 2*e*, undergoes the refraction NR2*e*; and the ray WGR, which belongs to the axis WCO, and is refracted

by

by the prism PR to *w*, undergoes the refraction OR*w*. Now NC2*e*, part of the angle measured, is to NR2*e*, the refraction of the prism RS, as R*w* to C*w*; and OC*w*, the other part of the angle measured, is to OR*w*, the refraction of the prism PR, in the same ratio of R*w* to C*w*: therefore OCN, the whole angle measured, is to ORN, the sum of the refractions of the two prisms, as R*w* to C*w*; that is, as the distance of the prisms from the focus of the object-glass to the focal distance of the object-glass.

When the prisms are placed in the manner represented in fig. 3. the point *e* of the image *we* is illuminated only by the rays which fall on the object-glass between A and F, and the point 2*w* only by the rays which fall on the object-glass between B and G. Now the angles CRF, CRG, equal to the refractions of the prisms, being constant, the spaces FC, CG, will increase in proportion as the distances RF, RG, increase, and the spaces AF, GB, diminish as much; and therefore, the images at the point of mutual contact *e*2*w* will be each illuminated by half the rays which fall on the object-glass when the prisms are placed close to the object-glass; but will be enlightened less and less the nearer the prisms are brought to the focus of the object-glass.

But

But when the prifms are placed in the manner fhewn in fig. 4. the images at the point of contact, as the prifms are removed from the object-glafs towards the eye-glafs, will be enlightened with more than half the rays that fall on the object-glafs, and will be moft enlightened when the prifms are brought to the focus itfelf; for the point *2e* of the image *2w2e* will be enlightened by all the rays EE that fall on the object-glafs between B and F, and the point *w* of the image *we* will be enlightened by all the rays WW which fall on the object-glafs between A and G. But the difference of the illuminations is not very confiderable in achromatic telefcopes, on account of the great aperture of the object-glafs; as the greateft fpace FG is to the focal diftance of the object-glafs, as the fum of the fines of the refractions of the prifms is to the radius.

There is a third way, and perhaps the beft, of placing the prifms, fo as to touch one another along their fides which are at right angles to the common fections of their refracting planes. In this difpofition of the prifms, the images will be equally enlightened, namely, each with half the rays which fall on the object-glafs, wherever the prifms be placed between the object-glafs and eye-glafs.

From

From what has been fhewn it appears, that this inftrument, which may be properly called the prifmatic micrometer, will meafure any angle that does not exceed the fum of the refractions of the prifms, excepting only very fmall angles, which cannot be taken with it on account of the vanifhing of the pencils of rays at the juncture of the two prifms near the focus of the object-glafs; that it will afford a very large fcale, namely, the whole focal length of the object-glafs for the greateft angle meafured by it; and that it will never be out of adjuftment; as the point of the fcale where the meafurement begins (or the point of o) anfwers to the focus of the object-glafs, which is a fixed point for celeftial objects, and a point very eafily found for terreftrial objects. All that will be neceffary to be done, in order to find the value of the fcale of this micrometer, will be to meafure accurately the diftance of the prifms from the focus when the inftrument is fet to meafure the apparent diameter of any object fubtending a known angle at the centre of the object-glafs, which may be eafily found by experiment, as by meafuring a bafe and the diameter of the object obferved placed at the end of it, in the manner practiced with other micrometers: for the angle fubtended by this object will be to the angle fubtended by a celeftial object, or very remote land object, when the

 diftance

diſtance of the priſms from the principal focus is the ſame as it was found from the actual focus in the terreſtrial experiment, as the principal focal diſtance of the object-glaſs is to the actual focal diſtance in the ſaid experiment.

It will, I apprehend, be the beſt way in practice, inſtead of one priſm to uſe two priſms, refracting contrary ways, and ſo divide the refraction between them (as repreſented in fig. 3. and 4.). Achromatic priſms, each compoſed of two priſms of flint and crown-glaſs, placed with their refracting angles contrary ways, will undoubtedly be neceſſary for meaſuring angles with great preciſion by this inſtrument: and I can add with pleaſure, that I find by experiment made with this inſtrument, as it was executed by Mr. DOLLOND with achromatic priſms, ground with great care for this trial above a twelve-month ago, that the images after refraction through the priſms appear very diſtinct; and that obſervations of the apparent diameters of objects may be taken in the manner here propoſed with eaſe and preciſion.

Two or more ſets of priſms may be adapted to the ſame teleſcope, to be uſed each in their turn, for the more commodious meaſurement of different angles. Thus it may be very convenient to uſe one ſet of priſms for mea-

furing angles not exceeding 36′, and confequently fit for meafuring the diameters of the Sun and Moon, and the lucid parts and diftances of the cufps in their eclipfes; and another fet of prifms to meafure angles not much exceeding one minute, and confequently fit for meafuring the diameters of all the other planets. This latter fet of prifms will be the more convenient for meafuring fmall angles, on account of a fmall imperfection attending the ufe of this micrometer, as before mentioned; namely, that angles cannot be meafured with it when the prifms approach very near the focus of the object-glafs, the pencils of rays being there loft at the point where the prifms touch one another.

Upon the principles that have been here explained, a prifm placed within the telefcope of an aftronomical inftrument, adjufted by a plumb-line or level, to receive all the rays that pafs through the object-glafs, may conveniently ferve the purpofe of a micrometer, and fuperfede the ufe both of the vernier fcale and the external micrometer; and the inftrument may then be always fet to fome even divifion before the obfervation. Thus the ufe of a telefcopic level may be extended to meafure with great accuracy the horizontal refractions, the depreffion of the horizon of the fea, and fmall altitudes and depreffions of land objects. Time and experience

 will

will doubtlefs fuggeft many other ufeful applications of this inftrument.

A paper from the learned Abbé BOSCOVICH was read before this Society the ninth of laft June, defcribing a fimilar contrivance as an invention of the Abbé ROCHON, in which the Abbé BOSCOVICH himfelf alfo claims fome fhare; I therefore defire to acquaint this Society, that I communicated this invention to Mr. DOLLOND, and had it executed by him; and alfo fhewed the inftrument itfelf, fo executed, to my efteemed friend ALEXANDER AUBERT, efq. fellow of this Society, a gentleman very well qualified to judge of things of this nature, above a twelvemonth before the communication of the Abbé BOSCOVICH'S paper, as will appear from their written atteftations, drawn up at my defire, defcribing the particulars of the communication of this invention which I made to them fo long ago. May I be permitted to remark, that this inftrument having been executed by my directions, in feveral forms, by Mr. DOLLOND, between the months of March and Auguft, 1776, and fet up and tried at his houfe in the prefence of feveral of his workmen, could not be confidered as an abfolute fecret concealed from the public. However, I doubt not that the following atteftations of Mr. AUBERT and Mr. DOLLOND will fufficiently prove my title to this invention of the prifmatic micro-

 meter;

meter; and I take this opportunity of exhibiting to the Society the instrument itself, mentioned in Mr. DOLLOND's letter as executed by himself according to my directions, and sent to the Royal Observatory in the month of August 1776.

Greenwich,
December 11, 1777.

TO THE REV. DR. MASKELYNE.

St. Paul's Church-yard,
Nov. 22, 1777.

REV. SIR,

ACCORDING to your desire I send the following particulars of the experiments which were made by your directions, for compleating a new kind of micrometer for measuring small angles. About the beginning of April 1776, I received your first directions respecting this matter, which were to make two prismatic glasses or wedges of such angles that rays of light, which passed through them, should be refracted about 18′ of a degree: these were to be placed between the object-glass and eye-glass of an achromatic telescope about 30 inches long. The angular edges of the two prismatic glasses were to be placed in contact with each other; they were to be moved in a parallel position from the object-glass to the focus

focus of the eye-glaſs, and to be of ſuch a ſize as to cover the aperture of the object-glaſs when brought cloſe to it. By the refraction of theſe wedges two images were formed in the teleſcope, which were at the greateſt diſtance (about 36′) when the wedges were cloſe to the object-glaſs, and approached as they were moved towards its focus, where they united; ſo that the whole focal diſtance of the object-glaſs was to be the length of the ſcale for meaſuring the angular diſtance of the two images formed in the teleſcope. When theſe wedges were applied, as above deſcribed, the two images were found to be coloured to a great degree, occaſioned by the refraction of the wedges. This defect you directed me to remove by making the priſmatic glaſſes or wedges achromatic, on the ſame principles as the achromatic object-glaſſes; and, after ſome difficulties, this was effected; the two images formed in the teleſcope appeared free from colours and diſtinct. The above experiments were made in a rough wooden tube, with an inconvenient method of moving the wedges by hand: in this ſtate it was when ſhewn to ALEXANDER AUBERT, eſq. F. R. S. towards the end of May, 1776; after which you deſired to have it done in a more compleat manner, in a braſs tube, with a means of turning the tube round to take angles in different directions, and a method of moving

moving the wedges with a screw. This was compleated about the middle of August in the same year, and then sent to the Royal Observatory. I have the honour to be,

REVEREND SIR,

Your obedient humble servant,

PETER DOLLOND.

I HEREBY certify, that in the month of May, 1776, the Rev. Mr. MASKELYNE, Astronomer-royal, produced to me, at Mr. DOLLOND's house in St. Paul's church-yard, and in his presence, as a new invention of his own, an instrument for measuring small angles, consisting of two achromatic prisms or wedges applied between the object-glass and eye-glass of an achromatic telescope about 30 inches long, by moving of which wedges nearer to, or farther from, the object-glass, the two images of an object produced by them appeared to approach to, or recede from, each other, so that the focal length of the object-glass became a scale for measuring the angular distance of the two images.

London,
Nov. 27, 1777.

ALEX[R]. AUBERT.

XXXVII. *The Report of the Committee appointed by the Royal Society to consider of the best Method of adjusting the fixed Points of Thermometers; and of the precautions necessary to be used in making Experiments with those Instruments.*

Read June 19, and Dec. 28, 1777.

IT is universally agreed by all those who make and use FAHRENHEIT's thermometers, that the freezing point, or that point which the thermometer stands at when surrounded by ice or snow beginning to melt, is to be called 32°; and that the heat of boiling water is to be called 212°: but for want of further regulations concerning the manner in which this last point is to be adjusted, it is placed not less than two or three degrees higher on some thermometers, even of those made by our best artists, than on others. The two principal causes of this difference are, first, that it has never been settled at what height of the barometer this point is to be adjusted[a]; and

(a) FAHRENHEIT found that the heat of boiling water differed according to the height of the barometer; but supposed the difference to be much greater than

and fecondly, that fo much of the quickfilver in the thermometer as is contained in the tube, is more heated in the method ufed by fome perfons, than in that ufed by others. To fhew that this laft circumftance ought by no means to be difregarded, fuppofe that the ball of a thermometer be dipped into boiling water as far as to the freezing point, and confequently that the length of the column of quickfilver in that part of the tube which is not immerfed in the water be 180°; and fuppofe that the heat of that part of the column of quickfilver be no more than 112°. If the thermometer be now intirely immerfed in the water, the heat of this column will be increafed 100°; and confequently its length will be increafed by $\frac{100}{11500}$ parts of the whole, as quickfilver expands $\frac{1}{11500}$ part of its bulk by each degree of heat; and confequently the thermometer will ftand $\frac{180 \times 100}{11500}$ or rather more than $1^{\circ}\frac{1}{2}$ higher than it did before.

Another thing to be confidered in adjufting the boiling point is, that if the ball be immerfed deep in the water, it will be furrounded by water which will be com-

than it really is. Mr. DE LUC has fince, by a great number of experiments made at very different heights above the level of the fea, found a rule by which the difference in the boiling point, anfwering to different heights of the barometer, is determined with great exactnefs. According to this rule the alteration of the boiling point by the variation of the barometer from $29\frac{1}{2}$ to $30\frac{1}{2}$ inches is $1^{\circ}.59$ of FAHRENHEIT.

preffed by more than the weight of the atmofphere, and on that account will be rather hotter than it ought to be.

We are of opinion, that the quickfilver in the tube ought, if poffible, to be kept of the fame heat as that in the ball, and that the ball ought not to be immerfed deep in the water. Thefe two requifites may be obtained by ufing a veffel covered fo as to allow no more paffage than what is fufficient for carrying off the fteam; for then, if the thermometer be inclofed in this veffel in fuch manner that the boiling point fhall rife but a little way above the cover, almoft all the quickfilver in the tube will be furrounded by the fteam of the boiling water, and confequently will be nearly of the fame heat as the water itfelf: we therefore made fome experiments to determine how regular the boiling point would be when tried in fuch veffels, both when the ball was immerfed in the water, and when it was expofed only to the fteam as recommended by Mr. CAVENDISH (b)

The veffel ufed in thefe experiments is reprefented in fig. 1. AB*ba* is the pot containing the boiling water; D*d* is the cover; E is a chimney for carrying off the fteam; M*m* is the thermometer faftened to a brafs frame; this thermometer is paffed through a hole F*f* in the cover, and refts thereon by a circular brafs plate G*g* faftened

(b) Phil. Tranf. vol. LXVI. p. 380.

to

to its frame, a piece of woollen cloth being placed between Gg and the cover, the better to prevent the escape of the vapours.

There were two pots of this kind used by us; one five inches in diameter and nine deep; the other, $4\frac{1}{4}$ in diameter and 23 deep. Two of the thermometers principally used were the short ones, the brass plate (Gg) being placed only $3\frac{3}{4}$ inches above the top of the ball, and the boiling point rising not much above that plate: the third thermometer was much longer, the plate (Gg) being 17 inches above the ball. They were all three quick; the first containing only $2\frac{1}{2}$ degrees to an inch; the second 5°; and the third 10°. The first had a cylinder instead of a ball $1\frac{1}{2}$ inch long and $\frac{4}{10}$ in diameter[(c)]; the two others had spherical balls, about $\frac{3}{4}$ of an inch in diameter.

On trying these thermometers in the above mentioned vessels, with the water rising two or three inches above the top of the ball, we found some variations in the height according to the different manner of making the experiment, but not very considerable; for the most part there was very little difference whether the water boiled

(c) In the two short thermometers the quicksilver would have descended into the ball when cold, had not the tube been swelled a little, close to the ball, in order to prevent it.

faſt or very gently; and what difference there was, was not always the ſame way, as the thermometer ſometimes ſtood higher when the water boiled faſt, and ſometimes lower. The difference, however, ſeldom amounted to more than $\frac{1}{10}$th of a degree, unleſs a conſiderable part of the ſides of the pot were expoſed to the fire; but in ſome trials which we made with the ſhort thermometers in the ſhort pot, with near four inches of the ſide of the veſſel expoſed to the fire(d), they conſtantly ſtood lower when the water boiled faſt than when ſlow, and the height was in general greater than when only the bottom of the pot was expoſed to the fire. This difference however was not perceived in the trials of the long thermometer in the deep pot, as there ſeemed very little difference in the height whether the water boiled faſt or ſlow, or whether more or leſs of the ſide of the pot was expoſed to the fire. The greateſt difference obſerved in the ſame thermometer, on the ſame day and in the ſame water, according to the different manner of trying the experiment, was half a degree.

(d) In all our experiments, the water was boiled over a portable black-lead furnace, covered with an iron plate, which had a hole cut in it juſt big enough to receive the bottom of the pot; ſo that, by paſſing the bottom through this hole to a greater or leſs depth, we could expoſe more or leſs of the ſides to the fire. In the other experiments, not more than one inch of the ſides was ever expoſed to the fire.

We

We made ſome trials with the long thermometer in the deep pot, to determine how much the height of the boiling point was affected by a greater or leſs depth of water above the ball. By a mean of the experiments it ſtood ,66 of a degree higher when the water roſe 15 inches above the ball, than when it was only three inches above the ball; ſo that increaſing the depth of water above the ball by 11 inches, raiſed the thermometer ,66 of a degree, that is ,06 for each inch.

We would by no means infer however from hence, that it is a conſtant rule, that the height of the boiling point is increaſed ,06 of a degree by the addition of each inch in the depth of the water above the ball; as perhaps the proportion would be found very different in greater depths of water or in wider veſſels.

If this rule is conſtant, it would ſhew that, when the preſſure on that part of the water which ſurrounds the ball is increaſed by increaſing the depth of water above the ball, the height of the boiling point is not altered thereby more than half as much as by an equal increaſe of preſſure produced by an alteration in the weight of the atmoſphere: for the preſſure on that part of the water which ſurrounds the ball is as much increaſed by an alteration of 11 inches in the depth of the water above the ball, as by an increaſe of $\frac{11}{13\frac{1}{2}}$ of an inch in the height

of

of the barometer; and ſuch an alteration in the height of the barometer is ſufficient to raiſe the boiling point 1°,3.

It ſeems as if the height of the boiling point was in ſome meaſure increaſed by having a great depth of water below the ball, as in general the ſhort thermometers ſtood higher when tried in the deep pot than in the ſhort one; this effect, however, did not always take place. In the former of theſe caſes, the depth of water below the ball was about 18 inches, in the other only 4; but the depth of water above the ball was the ſame in both caſes.

It muſt be obſerved, that when there was a great depth of water in the veſſel, either above or below the ball, the experiments were much more irregular, and the quickſilver in the tube remained much leſs ſteady than when it was ſmall. When the depth of water in the veſſel is great, it is apt to boil in guſts, which ſeems to be the cauſe of this irregularity; though we could not perceive any regular connection between theſe guſts and the riſing of the thermometer.

In the experiments made with the water not riſing ſo high as the ball, ſo that the thermometer was expoſed only to the ſteam, we very ſeldom found any ſenſible difference whether the water boiled faſt or ſlow: but whenever

whenever there was any, the greater height was when the water boiled faſt; the difference, however, never amounted to more than $\frac{1}{20}$th of a degree.

There was ſcarce ever any ſenſible difference whether the ſhort thermometers were tried in the ſhort pot or the deep one, though in the former caſe the ball was raiſed very little above the ſurface of the water, and in the latter not leſs than 14 inches: neither did we find any ſenſible difference in trying them in the tall pot, whether there was a greater or leſs depth of water in the veſſel.

As it was nevertheleſs ſuſpected, that the heat of the ſteam might poſſibly be leſs near the top of the pot than lower down (for in theſe experiments the ball of the thermometer was always at the ſame depth below the cover, though its height above the ſurface of the water was very different) we made two holes in the ſide of a pot four inches deeper than the deepeſt of the foregoing, one near the top of the pot, and the other not far from the bottom, and paſſed the ball of the thermometer through one or the other of theſe holes, taking care to ſtop up both holes very carefully, ſo that no air could enter into the pot by them: no ſenſible difference could be perceived in the height, whether the thermometer was placed in the upper or lower hole, though in one

cafe the ball was only three inches, and in the other 21 inches, below the cover.

The heat of the fteam therefore appears to be not fenfibly different in different parts of the fame pot; neither does there appear to be any fenfible difference in its heat, whether the water boil faft or flow; whether there be a greater or lefs depth of water in the pot; or whether there be a greater or lefs diftance between the furface of the water and the top of the pot; fo that the height of a thermometer tried in fteam, in veffels properly clofed, feems to be fcarce fenfibly affected by the different manner of trying the experiment.

Though, as was before faid, there was fcarce any difference in the height of the quickfilver, whether the water boiled faft or flow, yet, when the water boiled flow, the thermometer was a great while before it rofe to its proper height; and when it boiled very flow, it feemed doubtful whether it would have ever rifen to it, efpecially if the ball was raifed a great way above the furface of the water: but when, by making the water boil brifkly, the thermometer had once rifen to its proper height, the water might then be fuffered to boil very gently, even for a great length of time, without the thermometer finking fenfibly lower[e].

All

(e) The reafon of this feems to be that, while any air is left in the pot, the fteam

All three thermometers were found to ſtand, in general, from 30 to 65 hundredths of a degree higher when the ball was immerſed a little way in the water (neglecting thoſe obſervations in which much of the ſides of the pot were expoſed to the fire) than when it was tried in ſteam: at a medium they ſtood $\frac{48}{100}$ higher, which is equal to the difference produced by a variation of $\frac{3}{10}$ths of an inch in the barometer; ſo that the boiling point, adjuſted at a given height of the barometer, with the ball immerſed a little way in the water, will in general agree with that adjuſted in ſteam, when the barometer is $\frac{3}{10}$ths of an inch higher.

It muſt be obſerved, that in all theſe experiments a piece of flat tin plate was laid looſely on the mouth of the chimney E, ſo as to leave no more paſſage for the ſteam than what was ſufficient to prevent the tin plate from being lifted up. In trying the thermometers in ſteam, this is by no means unneceſſary; for, if the cover of the pot does not fit pretty cloſe, the thermometers will immediately ſink ſeveral degrees on removing the tin plate; but, when their balls are immerſed in the water, the removal of the tin plate has no ſenſible effect.

ſteam cannot acquire its full degree of heat; and that when the water boils very gently, the air is not eaſily intirely expelled from the pot. That the ſteam will not acquire its full degree of heat while any air is left in the pot will appear from the next paragraph but one.

If this cover to the chimney had been heavy, the included ſteam might have been ſo much compreſſed thereby, that the water and ſteam might have acquired a conſiderably greater heat than they ought to have done; but as this plate lay looſe on the chimney, and as its weight was not greater than that of a column of quickſilver, whoſe baſe is equal to that of the mouth of the chimney, and whoſe altitude is $\frac{1}{50}$th of an inch, the exceſs of the compreſſion of the included ſteam above that which it would ſuffer in an open veſſel, could not be greater than that which would be cauſed by an increaſe of $\frac{1}{50}$th of an inch in the height of the barometer, which is too ſmall to be worth taking notice of; for, if the exceſs of compreſſion was greater than that, the tin plate muſt neceſſarily be lifted up ſo much as to afford a ſufficient paſſage for the ſteam to eſcape faſt enough, though urged by no greater force than that.

Though in the different trials of the ſame thermometer in ſteam, on the ſame day, and with the ſame water, ſo little difference was obſerved, according to the different manner of trying the experiment; yet there was a very ſenſible difference between the trials made on different days, even when reduced to the ſame height of the barometer, though the obſervations were always made either with rain or diſtilled water. The difference,

however, never amounted to more than a quarter of a degree, except in one thermometer, in which there were three obſervations out of eighteen which differed more than that; one of them differed ſo much as 0.65° from ſome of the reſt. In the obſervations made with the ball immerſed a little way in the water, there was a greater difference between the obſervations of different days, even neglecting thoſe in which much of the ſides of the pot were expoſed to the fire. In two of the thermometers the different obſervations differed about $\frac{35}{100}$ of a degree from each other; but in the other thermometer they varied $\frac{8}{10}$ths.

We do not at all know what this difference could be owing to, eſpecially in the obſervations in ſteam. It could not proceed intirely from ſome unknown difference in the water; for, if it did, the difference between the different thermometers ſhould have been always the ſame, which was not the caſe, though in general, on thoſe days in which one thermometer ſtood high, the others did alſo, eſpecially in the trials in ſteam. Moreover, as far as can be perceived from our experiments, there ſeems to be very little difference between different waters with reſpect to the heat which they acquire in boiling. We could not be ſure that there was any difference between rain or diſtilled water and pump water, provided

the latter had boiled long: neither did any difference feem to arife from the water containing fuch fubftances as are difpofed to part readily with their phlogifton; for, on trying the thermometers in the fteam of diftilled water, their height was not fenfibly altered by pouring in a fmall quantity of a folution of liver of fulphur, or of iron filings imperfectly rufted. The thermometer, however, feemed to ftand fenfibly lower in pump water beginning to boil, than in the fame water long boiled, but the difference fcarcely exceeded $\frac{1}{10}$th or $\frac{1}{5}$th of a degree.

We made fome experiments to determine the heat of water boiling in open veffels. In general, when the veffel was almoft full, and the water boiled faft, and the ball of the thermometer was held from three-quarters to two or three inches under water, and alfo in that part of the veffel where the current of water afcended upwards, that is, in the hotteft part of the water, its heat was not much different from that of the fteam of water boiling in clofed veffels, varying only from a quarter of a degree more than that, to as much lefs; but if the water boiled gently, its heat would frequently be half or three-quarters of a degree cooler than the fteam. If the experiment was tried in the deep pot with fuch a quantity of water in it that the furface was at leaft 14 or 15 inches below the top of the pot, fo that though the veffel was open,

open, yet the water was not much expofed to the air, its heat then feemed fcarcely lefs than when boiled in clofed veffels.

In making thefe experiments we chiefly made ufe of the two fhort thermometers, in which, as the quantity of quickfilver contained in the tube was fmall, the error arifing from that part of the quickfilver being not heated equally with that in the ball, could be but fmall: for example, in the fecond of the fhort thermometers, the number of degrees contained in that part of the tube between the circular plate G*g* and the ball was 18°. In the experiments in fteam this part of the tube was heated to the fame degree as the ball. Suppofe now, that in open veffels it was heated only to 122°, or was 90° cooler than the ball, it is plain, that the thermometer would ftand only $\frac{18 \times 90}{11500}$, or $\frac{1}{7}$th of a degree lower than it did in fteam, provided the heat of the quickfilver in the ball was the fame in both cafes. In the other fhort thermometer, as there were only half as many degrees to an inch, the error was only half as great.

In feveral of the experiments, however, we made ufe of the long thermometer; but then it was neceffary to make an allowance on account of the quickfilver in the tube being not heated equally with that in the ball. The

better

better to enable us to do this, we made uſe of a thermometer tube, filled with quickſilver in the ſame manner as a thermometer, only without any ball to it, or a thermometer without a ball, as we may call it. A ſmall braſs plate was fixed to the tube near the top of the column of quickſilver, to ſhew the heat as in a common thermometer. In all our experiments with the long thermometer in open veſſels, this tube without a ball, was placed by its ſide; whence, as the quickſilver in the tube of the long thermometer could hardly fail of being nearly of the ſame heat as that in the tube without a ball, we knew pretty nearly the heat of the quickſilver in the tube of the former, and conſequently how much higher it would have ſtood if the quickſilver in its tube had been of the ſame heat as that in the ball. For example, on October 19, the long thermometer tried in an open veſſel, the water boiling faſt, ſtood 1°.65 lower than it did when tried in ſteam the ſame day, the quickſilver in the tube without a ball ſtanding at the ſame time at 109°: we may therefore conclude, that the heat of the quickſilver, in that part of the tube of the long thermometer which was not immerſed in the water, was alſo 109°; and conſequently, as that part of the tube contained about 170°, the thermometer ſtood $\frac{170 \times 103}{11500}$, or

1°52

1°.52 lower than it would have done if the quickfilver in the tube had been of the fame heat as that in the ball; and, confequently, the quickfilver in the ball of the thermometer was in reality .07 cooler than when tried in fteam.

We examined the boiling points of feveral thermometers, made by different artifts, by trying them in fteam when the barometer was at 30.1, and finding what divifion on the fcale the quickfilver ftood at. The difference of the extremes was $3°\frac{1}{4}$; but, by a mean of all, it was found to ftand at 213°.1, and confequently would have ftood at 212°, if the barometer had been at 29.4; fo that if the boiling point was to be adjufted, either in fteam, when the barometer is at 29.4, or with the ball immerfed two or three inches in water, when the barometer is at 29.1, it would agree beft with the mean of the abovementioned thermometers. But as it feems to be of no great fignification to make the boiling point agree very nearly with the mean of the thermometers made at prefent, when the extremes differ fo widely; and as we apprehend that it will be more convenient to the makers that fome height fhould be chofen which differs lefs from the mean, as thereby they will more frequently have an opportunity of adjufting the boiling point without the trouble and danger of miftakes which attend the

making

making a correction, we recommend, that the boiling point ſhould be adjuſted when the barometer is at 29.8, if the perſon chuſes to do it in ſteam; or when the barometer is at $29\frac{1}{2}$, if he chooſes to do it in cloſe veſſels, with the ball immerſed to a ſmall depth under the water. Our reaſon for pitching upon this preciſe height is, that thereby the boiling point will differ from Mr. DE LUC's boiling point, by a ſimple fraction of the degrees of his common ſcale, namely three-quarters of a degree higher.

We are informed by Mr. DE LUC, that the method he uſed in adjuſting the boiling point, though he forgot to mention it in the *Récherches ſur les Modifications de l'Atmoſphere*, was to wrap rags round the tube of the thermometer, and to try it with the ball immerſed in water in an open veſſel, of the form deſcribed in the abovementioned book, while boiling water was poured at different times on the rags, in order that the quickſilver in the tube might be heated, if poſſible, to the ſame degree as that in the ball. As well as we can judge from the abovementioned experiments in open veſſels, and from the few trials we have made of this method, we are inclined to think, that the boiling point adjuſted this way will in general differ but little from that adjuſted in ſteam at the ſame height of the barometer, eſpecially if the thermometer be not very long, and do not extend a great

way

way below the freezing point(f); consequently, as Mr. DE LUC's boiling point was adjusted when the barometer was at 27 Paris or 28.75 English inches, it will stand lower than that adjusted in the manner recommended by us, by three-quarters of a degree of his scale; or $80°\frac{3}{4}$ on DE LUC's thermometer, will answer to 212° on FAHRENHEIT's adjusted in the manner proposed.

Though the boiling point be placed so much higher on some of the thermometers now made than on others, yet we would not have the reader think that this can make any considerable error in the observations of the weather, at least in this climate; for an error of $1°\frac{1}{2}$ in the position of the boiling point will make an error of only half a degree in the position of 92°, and of not more than a quarter of a degree in the point of 62°. It is only in nice experiments, or in trying the heat of hot liquors, that this error in the boiling point can be of much signification.

(f) In order to see how much the quicksilver in the tube of the thermometer would be heated in this method of adjusting the boiling point, we took the abovementioned tube without a ball, wrapped it round with rags, and poured boiling water on it as above described: the heat of the quicksilver therein was found to be about 21° less than that of boiling water; and, therefore, the boiling point of a thermometer, adjusted in this manner, supposing the thermometer to be dipped into the water as far as to the point of 32°, should stand about one-third of a degree lower than it would do if the quicksilver in the tube was heated equally with that in the ball.

 There

There is another circumſtance that we have not yet taken notice of, which, in ſtrictneſs, cauſes ſome error in thermometers, namely, the difference of expanſion of the glaſs tube and the ſcale. But this error is in almoſt all caſes ſo ſmall as to be not worth regarding; we have, however, in the note below given a rule for computing the value of it (g).

(*g*) The uſual way of adjuſting thermometers is, to mark the boiling and freezing points on the glaſs tube, and not to ſet off thoſe points on the ſcale till ſome time after, when the tube and ſcale may both be ſuppoſed to be nearly of the temper of the air in the room; conſequently, when the thermometer is expoſed to a greater heat than that, the ſcale, if of braſs, will expand more than the glaſs tube, and the diviſions on it will be longer than they ought to be; but, if the ſcale be of wood, it will expand leſs than the glaſs tube, and the diviſions will be too ſhort. Let now the heat of the air, when the diviſions were ſet off on the ſcale, be called A; let the degree of heat which the thermometer ſtands at in the experiment be called D; and let the degree anſwering to that point of the ſcale in which the thermometer is faſtened to the ſcale be called F. Then, if all parts of the thermometer and ſcale are heated equally, and the ſcale is of braſs, the thermometer will appear to ſtand lower than it ought to do by the $\frac{\overline{D-F} \times \overline{D-A}}{165000}$ part of a degree, obſerving, that if $\overline{D-F} \times \overline{D-A}$ is negative, it will ſtand higher than it ought to do; but if the ſcale is of wood, it will ſtand higher than it ought to do by the $\frac{\overline{D-F} \times \overline{D-A}}{216000}$ part of a degree. If the thermometer be faſtened to the ſcale by the ball, or any part of the tube lower than the obſerved heat, the error will be the ſame, whether that part of the tube and ſcale, which is above the obſerved degree, be of the ſame heat as the ball or not: but if the thermometer is faſtened to the ſcale by the top of the tube, as is frequently done, then the error will vaniſh whenever that part of the tube and ſcale, which is above the obſerved degree, is not much heated. This rule is founded on Mr. SMEATON's experiments, who found, that white glaſs expands $\frac{1}{100}$th of an inch in a foot by 180° of heat; that braſs wire expands $\frac{232}{10000}$; and that wood expands ſcarce ſenſibly.

In

In making experiments with thermometers, it evidently is equally neceſſary that the quickſilver in the tube ſhould be of the ſame heat as that in the ball, as it is in adjuſting the boiling point: for this reaſon, in trying the heat of liquors much hotter or colder than the air, the thermometer ought, if poſſible, to be immerſed as far as to the top of the column of quickſilver in the tube. As this, however, would often be very difficult to execute, the obſerver will frequently be obliged to content himſelf with immerſing it to a much leſs depth. But then as the quickſilver, in a great part of the tube, will be of a different heat from that in the ball, it will be neceſſary, where any degree of accuracy is required, to make a correction, on that account, to the heat ſhewn by the thermometer. If the heat of the quickſilver in the tube be known, the correction may readily be made by help of the annexed table; the only difficulty lies in eſtimating what that heat may be. In all probability the heat of the quickſilver in the tube will not be very different from that of the air which ſurrounds it[(h)]; but as that

(h) This muſt evidently be the caſe, unleſs the quickſilver in the tube is conſiderably heated by its contact with that in the ball. To ſee whether this was the caſe, ſome ſand was heated in a ſmall copper diſh over a lamp to the heat of about 212°, and the abovementioned tube, without a ball, laid horizontal with the end extending about half an inch over the ſand; but, to prevent its being heated thereby, a piece of wood, about a quarter of an inch thick, was

that air will be affected by the steam of the liquor, and the fire by which it is heated, it will commonly be of a very different heat from the rest of the air of the room in which the experiment is made; but as no great nicety is required in estimating the heat of the quicksilver in the tube, insomuch that a mistake of 25° therein will cause an error of only half a degree in the correction, when the number of degrees in that part of the tube which is not immersed in the liquor is not more than 220°, it will commonly be not difficult to guess at the heat of the quicksilver in the tube as near as is required(i). But if the observer is desirous of more accuracy,

was laid between the sand and it. After it had remained a sufficient time in this situation, the division which the quicksilver stood at was observed. The piece of wood was then removed, and the end of the tube laid in the sand, which was heaped over it so that about half an inch of the column of quicksilver was intirely surrounded by the hot sand, and must therefore be heated to nearly the same degree as it. The quicksilver in the tube rose very little higher than before, and seemingly not more than might be owing to the expansion of the half inch of quicksilver which was surrounded by the sand; so that it should seem, that heating one end of the column of quicksilver does not communicate much heat to the rest of the column; and consequently, that, when the ball of a thermometer is immersed in hot liquor, the quicksilver in the tube will not be much hotter than the surrounding air.

(i) The better to enable the reader to guess at the heat of the quicksilver in the tube, in cases of this kind, we tried how much the quicksilver in the abovementioned tube, without a ball, would be heated when held over a vessel of boiling water. It is true, that these experiments cannot be of any great service towards this purpose, as the tubes will be very differently heated, according to the

racy, he may find the heat of the surrounding air by holding the ball of a small thermometer near the tube of

the degree of heat of the fluid, and the quantity of steam which it furnishes, and according to the nature of the fire by which it is heated; yet as the experiments may perhaps serve in some measure to rectify our ideas on this head, we will give the result. When the abovementioned tube without a ball *, the length of the column of quicksilver in which was 15 inches, was held perpendicularly over the vessel of boiling water, with its bottom even with the surface of the water, the heat of the quicksilver was in all the trials we made from 68 to 28° hotter than the air of the room. If the tube was held inclined to the horizon, in an angle of about 30°, with the bottom of the column of quicksilver reaching not more than three quarters of an inch within the circumference of the pot, so that the column of quicksilver was as little heated by the steam as could easily be done, it was from 30 to 7° hotter than the air. When a shorter tube of the same kind, in which the column of quicksilver was seven inches, was used, the quicksilver was from 62 to 44° hotter than the air, when held perpendicularly, and from 49 to 36° hotter when held inclined. The water in these trials frequently boiled pretty fast, but never very violently. It was in general heated over a portable black lead furnace placed in the middle of the room; but it was once heated over an ordinary chafing-dish, when the quicksilver in the long tube, held perpendicularly, was found to be 64° hotter than the air. When the experiments were tried without doors, the heat of the quicksilver in the tube would vary very much, according as the wind blew the steam and hot air from or towards the tube, but it sometimes rose as high as it did within doors.

The most convenient method we know of making these tubes without a ball is, to fill a thermometer in the usual manner, and heat the ball till there is a proper quantity of quicksilver in the tube, and then to make the column of quicksilver separate at the neck of the ball, and run to the extremity of the tube, so as to leave a vacuum between the ball and the column of quicksilver, as is expressed in fig. 2. where the shaded part AD represents the column of quicksilver, and BA that part in which there is a vacuum. The tube must then be sealed some-

* See p. 830. l. 1.

where

of the thermometer with which he tries the heat of the liquor; or, what will be much better, he may have a tube without a ball, ſuch as is above deſcribed, faſtened to the frame of the thermometer, on one ſide of the tube; or if he has two ſuch tubes, of different lengths, it will be ſtill more accurate.

To avoid the inconvenience of this correction, perhaps it may be thought, that both in adjuſting the boiling point and in trying the heat of liquors, it would be better that not much more than the ball of the thermometer ſhould be immerſed, and that the tube ſhould be held inclined in ſuch manner as to be heated as little as poſſible; as it may be ſaid, that by this means you will find the heat of liquors pretty nearly, without the trouble of making any correction; and that, though in ſtrictneſs a correction would be required in obſerving the heat of the air with

where between B and A as at E, and cut off there; after which it muſt be held with the end D upwards, ſo as to make the column of quickſilver run to the extremity E: by this method of filling it is plain, that no ſenſible quantity of air can be left between E and the column of quickſilver; but yet the quickſilver will be apt not to run ſufficiently cloſe to the extremity E, as the weight of the column will be ſcarcely ſufficient to force it into the narrow ſpace which will commonly be left in ſealing the tube, eſpecially when held nearly horizontal: for this reaſon it will be proper to open the tube at D, ſo as to let in the air, and then ſeal it again. It muſt be obſerved, that the ſpace left between D and the column of quickſilver ought not to be leſs than the tenth part of the length of the column of quickſilver, as otherwiſe the included air might be too much compreſſed by the expanſion of the quickſilver when much heated.

ſuch

ſuch thermometers, yet the heat of the atmoſphere never differs ſo much from the mean heat, as to make that correction of much conſequence(k). But, on the other hand, this method of making and uſing thermometers is much leſs exact than the former, and therefore is unfit for nice experiments; and, beſides, a correction would be as neceſſary with this kind of thermometer in trying the heat of air, artificially heated, or in finding the heat of large quantities of hot liquors, in which it would be difficult to prevent the quickſilver in the tube from being heated

(k) The degrees on all thermometers are intended to anſwer to equal portions of the ſolid contents of the tube; and, conſequently, if the quickſilver in the tube is kept conſtantly of the ſame heat as that in the ball, the degrees will anſwer to equal increments of bulk of the whole quantity of quickſilver in the thermometer, that is of a given weight of quickſilver. But if only the quickſilver in the ball is heated, and that in the tube is kept always of the ſame heat, the degrees will anſwer to equal increments of a given bulk of quickſilver; ſo that the ſcale of the thermometers will be really different in theſe two methods of proceeding, and in high degrees the difference will be very conſiderable: for example, let two thermometers be made, and in the firſt of them let care be taken, both in adjuſting the fixed points and in trying the heat of liquors, that the quickſilver in the tube ſhall be of the ſame heat as that in the ball; and in adjuſting the fixed points of the ſecond, and in trying the heat of liquors with it, let care be taken that the quickſilver in the tube ſhall remain always of the ſame invariable heat, and let the freezing and boiling points be marked 32 and 212 on both of them: then will the degree of 620 on the firſt anſwer to that of 600 on the ſecond; that of 406 to 400; that of 302 to 300; and that of 119.7 to 120; that is, a liquor which appears to be of 620° of heat by the firſt will appear to be of 600 by the ſecond, &c. It appears from hence, that it would be improper to employ the latter method of adjuſting and uſing thermometers for ordinary purpoſes, and the former for nice experiments.

by the fteam, as it is in finding the heat of liquors with the other thermometer, whenever the ball is not immerfed to a fufficient depth; fo that, on the whole, the former method of making and ufing thermometers feems much the beft.

A much better way of avoiding the trouble of making a correction would be to have two fets of divifions made to fuch thermometers as are intended for trying the heat of liquors; one of which fhould be ufed when the tube is immerfed almoft to the top of the column of quickfilver; and the other, when not much more than the ball is immerfed, in which laft cafe the obferver fhould be careful that the tube fhould be as little heated by the fteam of the liquor as conveniently can be. It is difficult to give rules for conftructing this fecond fet of divifions, as the heat of the quickfilver in the tube will be very different according to the temper of the air in the room, the quantity and nature of the fluid whofe heat is to be tried, the manner in which it is heated, and the other circumftances of the experiment; but, on the whole, we think that, given in the following table, would be as proper as any.

	Degree answering to that point of the tube which is two inches above the ball.										
	+75	+50	+25	0	—50	—100	—200	—300	—400	—500	
—500										—500	0
—400									—400	—396.5	0
—300								—300	—297.4	—294.8	0
—250								—248.9	—246.7	—244.6	0
—200							—200.	—198.3	—196.5	—194.8	0
—150							—149.3	—148.	—146 7	—145.4	0
—100						—100	— 98.9	— 97.7	— 96.6	— 95.5	30
— 50					—50	—49.7	— 49.	— 48.3	— 47.6	— 46.9	30
0				0	+0.2	+0.3	+ 0.6	+ 0.9	+ 1.2	+ 1.5	35
+150	149.5	149.4	149.	148.7	148.4	147.3					75
+200	198.8	198.5	198.3	198.	197.5	197.					85
+250	247.5	247.1	246.8	246.4	245.7						85
+300	295.8	295.3	294.9	294.4	293.4						85
+350	343.7	343.1	342.5	342.							85
+400	391.1	390.4	389.7	389.1							85
+450	438.1	437.3	436.5	435.7							85
+500	484.7	483.8	482.9	482.							85
+600	576.5	575.4	574.3	573.2							85

To make uſe of this table, ſeek in the uppermoſt horizontal line the degree of the thermometer anſwering to that point of the tube which is two inches above the ball; and in the left-hand column ſeek the degrees of the ſecond ſet of diviſions; the correſponding numbers in the table are the correſponding degrees of the firſt ſet, or the degrees which they muſt be ſet oppoſite to. The right-hand perpendicular column ſhews the heat which the quickſilver in the tube was ſuppoſed to be of in forming this table.

Though this ſecond ſet of diviſions be far from accurate, yet it is at leaſt as much ſo as a thermometer adjuſted in the latter method can be; ſo that this double ſet of diviſions poſſeſſes all the advantages which can be expected from that method of adjuſting thermometers, without the inconveniences.

A table

A table for correcting the obſerved height of a thermometer, whenever the quickſilver in the tube is not of the ſame heat as that in the ball.

Diff. of Heat	Degrees not immerſed in the liquors.													
	50	100	150	200	250	300	350	400	450	500	550	600	650	700	7
50	.2	.4	.7	.9	1.1	1.3	1.5	1.7	2.	2.2	2.4	2.6	2.8	3.1	3
100	.4	.9	1.3	1.8	2.2	2.6	3.0	3.5	3.9	4.4	4.8	5.2	5.7	6.1	6
150	.7	1.3	2.0	2.6	3.3	3.8	4.6	5.2	5.9	6.5	7.2	7.9	8.4	9.2	9
200	.9	1.8	2.6	3.5	4.4	5.1	6.1	7.0	7.8	8.7	9.6	10	11	12	1
250	1.1	2.2	3.3	4.4	5.5	6.4	7.6	8.7	9 8	11	12	13	14	15	1
300	1.3	2.6	3.8	5.1	6.4	7.7	9.1	10	12	13	14	16	17	18	2
350	1.5	3.0	4.6	6.1	7.6	9.1	11	12	14	15	17	18	20	21	2
400	1.7	3.5	5.2	7.0	8.7	10	12	14	16	17	19	21	23	24	2
450	2.	3.9	5.9	7.8	9.8	12	14	16	18	20	22	24	25	27	2
500	2.2	4.4	6.5	8.7	11	13	15	17	20	22	24	26	28	31	3
550	2.4	4.8	7.2	9.6	12	14	17	19	22	24	26	29	31	34	3

To make uſe of this table, in the left-hand perpendicular column look for the number of degrees contained in that part of the tube which is not immerſed in the fluid whoſe heat is to be tried, and in the upper horizontal line ſeek the ſuppoſed difference of heat of the quickſilver in that part of the tube from that in the ball; the correſponding number in the table is the correction, which muſt be added to the obſerved heat when the

 quick-

quickfilver in the tube is cooler than that in the ball, and fubtracted when it is warmer: for example, let the obferved heat of the fluid be 475°, let the thermometer be immerfed in the fluid as far as to the degree of 25°, or to that part of the tube which fhould be marked 25° if the divifions were continued long enough; then is the number of degrees in that part of the tube which is not immerfed in the fluid 450; and let the heat of the quickfilver in that part of the tube be fuppofed 100°; and confequently, the difference of heat of the quickfilver in that part of the tube from that in the ball 375; then in the left-hand perpendicular column feek the number 450, and in the upper horizontal line the number 375; the correfponding number in the table, or the correction, is 15°, and therefore the true heat of the fluid is 490°.

This correction may be had very eafily without the help of the table, only by multiplying the number of degrees not immerfed in the fluid by the fuppofed difference of heat, dividing the product by 10000, and diminifhing the quotient by one-eighth part of the whole.

In the following pages we have thrown together the practical rules, which we would recommend to be obferved in adjufting the fixed points of thermometers.

Rules to be observed in adjusting the boiling point.

The most accurate way of adjusting the boiling point is, not to dip the thermometer into the water, but to expose it only to the steam, in a vessel closed up in the manner represented in fig. 4. where AB*ba* is the vessel containing the boiling water, D*d* the cover, E a chimney made in the cover intended to carry off the steam, and M*m* the thermometer passed through a hole in the cover. Those who would make use of this method must take care to attend to the following particulars.

1st, The boling point must be adjusted when the barometer is at 29.8 inches; unless the operator is willing to correct the observed point in the manner directed below.

2dly, The ball of the thermometer must be placed at such a depth within the pot, that the boiling point shall rise very little above the cover; for otherwise part of the quicksilver in the tube will not be heated, and therefore the thermometer will not rise to its proper height. The surface of the water in the pot also should be at least one or two inches below the bottom of the ball; as otherwise the water, when boiling fast, might be apt to touch the ball: but it does not signify how much lower than that the surface of the water may be.

3dly,

3dly, Care muſt be taken to ſtop up the hole in the cover through which the tube is inſerted, and to make the cover fit pretty cloſe, ſo that no air ſhall enter into the pot that way, and that not much ſteam may eſcape. A piece of thin flat tin plate muſt alſo be laid on the mouth of the chimney, ſo as to leave no more paſſage than what is ſufficient to carry off the ſteam. The ſize of this plate ſhould be not much more than ſufficient to cover the chimney, that its weight may not be too great; and the mouth of the chimney ſhould be made flat, that the plate may cover it more completely. It muſt be obſerved, that when the tin plate is laid on the mouth of the chimney, it will commonly be lifted up by the force of the ſteam, and will rattle till it has ſlipped aſide ſufficiently to let the ſteam eſcape without lifting it up. In this caſe it is not neceſſary to put the plate back again, unleſs by accident it has ſlipped aſide more than uſual. If the artiſt pleaſes, he may tie each corner of this plate by a ſtring to prongs fixed to the chimney, and ſtanding on a level with the plate, as thereby it will neceſſarily be kept always in its place(l); but we would by no means recommend having it made with a hinge, as that might

(l) Fig. 3. is a perſpective view of the chimney and tin plate; ABCD is the plate; E the chimney; F*f*, G*g*, M*m*, and N*n*, the prongs faſtened to the chimney, to which the four corners of the plate are to be tied by the ſtrings AF, BG, CM, and DN; the ends F, G, M, and N, of the prongs muſt be on a level with the plate, and the ſtrings ſhould not be ſtretched tight.

5 be

be apt to make it ftick, in which cafe the included vapour might be fo much compreffed as to caufe an error. We would alfo by no means advife lining the tin plate with leather, or any other foft fubftance, for the fake of making it fhut clofer, as that alfo might be apt to make it ftick. The chimney alfo ought not to be made lefs than half a fquare inch in area: for though a fmaller chimney would be fufficient to carry off the fteam, unlefs the veffel is much larger than what we ufed; yet the adhefion which is apt to take place between it and the tin plate when wet, might perhaps bear too great a proportion to the power which the included fteam has to lift it off, if it was made much lefs. It is convenient that the chimney be not lefs than two or three inches long, as thereby the obferver will be lefs incommoded by the fteam; but it would be improper to make it much longer, for the longer the chimney is, the greater difpofition has the air to enter into the pot between it and the cover.

It is moft convenient not to make the cover fit on tight, but to take on and off eafily; and to wrap fome fpun cotton round that part of the cover which enters into the pot, in order to make it fhut clofer; or, what feems to anfwer rather better, a ring of woollen cloth may be placed under the cover, fo as to lie between the top

of

of the pot and it. Thefe methods of making the cover fhut clofe can be ufed more conveniently when the cover is made to enter within the pot, as in the figure, than when it goes on on the outfide.

There are various eafy ways by which the hole in the cover, through which the tube of the thermometer is paffed, may be ftopped up, and by which the thermometer may be fufpended at the proper height. The hole in the cover may be ftopped up by a cork, which muft firft have a hole bored through it, big enough to receive the tube, and be then cut into two, parallel to the length of the hole. Another method, more convenient in ufe, but not fo eafily made, is reprefented in fig. 6. which exhibits a perfpective view of the apparatus; A *a* is the cover; H the hole through which the thermometer is paffed; B *b* a flat piece of brafs fixed upon the cover; and D *d* E *e* a fliding piece of brafs, made fo as either to cover the hole H, or to leave it uncovered as in the figure, and to be tightened in either pofition by the fcrew S fliding in the flit M *m*; a femi-circular notch being made in the edge B *b*, and alfo in the edge D *d*, to inclofe the tube of the thermometer: pieces of woollen cloth fhould alfo be faftened to the edges B *b* and D *d*, and alfo to the bottom of the fliding piece D *d* E *e*, unlefs that piece and the cover are made fufficiently flat, to prevent the efcape of the fteam. In

order

order to keep the thermometer suspended at the proper height, a clip may be used like that represented in fig. 7. which by the screw s must be made to embrace the tube tightly, and may rest on the cover. That part of the clip which is intended to bear against the tube, had best be lined with woollen cloth, which will make it stick tighter to the tube, and with less danger of breaking it. Another method, which is rather more convenient, when the top of the tube of the thermometer is bent into a right angle, in the manner frequently practised at present for the sake of more conveniently fixing it to the scale, is represented in the same figure; G*g* F*f* is a plate of brass, standing perpendicularly on the cover, and L*l* N*n* a piece of brass, bent at bottom into the form of a loop, with a notch in it, so as to receive the tube of the thermometer, and to suffer the bent part to rest on the bottom of the loop; this piece must slide in a slit K*k*, cut in the plate L*l*N*n*, and be tightened at any height by the screw T.

4thly, It is best making the water boil pretty briskly, as otherwise the thermometer is apt to be a great while before it acquires its full heat, especially if the vessel is very deep. The observer too should wait at least one or two minutes after the thermometer appears to be stationary, before he concludes that it has acquired its full height.

 5thly,

5thly, Though, as was ſaid before, this appears to be the moſt accurate way of adjuſting the boiling point; yet, if the operator was to ſuffer the air to have any acceſs to the inſide of the veſſel, he would be liable to a very great error: for this reaſon we ſtrongly recommend it to all thoſe who uſe this method, not to deviate at all from the rules laid down without aſſuring themſelves, by repeated trials with a pretty ſenſible thermometer, that ſuch alteration may be uſed with ſafety. But the covering the chimney with the tin plate ought by no means to be omitted; for though, if the cover of the pot fits cloſe, it ſeldom ſignifies whether the plate is laid on or not, yet, if by accident the cover was not to fit cloſe, the omitting the tin plate would make a very great error. Making the chimney very narrow would not anſwer the end properly; for, if it was made ſo ſmall as to make the veſſel ſufficiently cloſe when the water boiled gently, it would not leave ſufficient paſſage for the eſcape of the ſteam when the water boiled faſt.

Another way of adjuſting the boiling point is, to try it in a veſſel of the ſame kind as the former, only with the water riſing a little way, namely from one to three or four inches above the ball, taking care that the boiling point ſhall riſe very little above the cover, as in the former method. In this method there is no need to cover the

the chimney with the tin plate; and there is lefs need to make the cover fit clofe, only it muft be obferved, that the clofer the cover fits, the lefs the operator will be incommoded by the fteam. The height of the barometer at which the boiling point fhould be adjufted, when this method is ufed, is $29\frac{1}{2}$ inches, or three-tenths of an inch lefs than when the former method is ufed.

It will be convenient to have two or three pots of different depths; for if a fhort thermometer is to be adjufted in the fame pot which is ufed for a long one, it will require a great depth of water, which, befides taking up more time before it boils, makes the obfervation rather lefs accurate, as the heat feems to be lefs regular when the depth of water in the pot is very great, than when it is lefs.

Perhaps fome perfons, for the fake of heating the water more expeditioufly, may be inclined to ufe an apparatus of fuch kind that the fire fhall be applied to a confiderable part of the fides of the pot as well as to the bottom; we would, however, caution them againft any thing of that kind, as the obfervations are confiderably lefs regular than when little more than the bottom of the pot is heated. If the pot is heated over a chafing-difh or common fire, we apprehend that there can feldom be any danger of too much of the fides being heated;

 but

but if the operator ſhould be apprehenſive that there is, it is eaſily prevented by faſtening an iron ring an inch or two broad round the pot near the bottom. This precaution is equally neceſſary when the thermometer is adjuſted in ſteam, eſpecially when there is not much water in the pot.

The greateſt inconvenience of this method of adjuſting the boiling point is the trouble of keeping a proper depth of water in the pot, as to do this it is neceſſary firſt to find the height of the boiling point coarſely by trying it in an open veſſel, and then to put ſuch a quantity of water into the pot that it ſhall riſe from one to three or four inches above the ball, when the thermometer is placed at ſuch a depth within the pot that the boiling point ſhall riſe very little above the cover. The operator muſt be very careful that the quantity of water in the pot be not ſo ſmall as not intirely to cover the ball.

A third way of adjuſting the boiling point is, to wrap ſeveral folds of linen rags or flannel round the tube of the thermometer, and to try it in an open veſſel, taking care to pour boiling water on the rags, in order to keep the quickſilver in the tube as nearly of the heat of boiling water as poſſible. The beſt way is to pour boiling water on the rags three or four times, waiting a few ſeconds between each time, and to wait ſome ſeconds after the laſt

time of pouring on water before the boiling point is marked, in order that the water may recover its full strength of boiling, which is in good measure checked by pouring on the boiling water.

In this method the boiling point should be adjusted when the barometer is at 29.8 inches, that is, the same as when the first method is used; the water should boil fast, and the thermometer should be held upright, with its ball two or three inches under water, and in that part of the vessel where the current of water ascends[(m)].

Whichever of these methods of adjusting the boiling point is used, it is not necessary to wait till the barometer is at the proper height, provided the operator will take care to correct the observed height according to the following table.

(*m*) In a vessel of boiling water one may almost always perceive the current of water to ascend on one side of the vessel, and to descend on the other.

Height

Height of the barometer when the boiling point is adjuſted according to,		Correction in 1000ths of the interval between 32° and 212°.		Height of the barometer when the boiling point is adjuſted according to,		Correction in 1000ths of the interval between 32° and 212°.	
1ſt or 3d method.	2d method.			1ſt or 3d method.	2d method.		
	30.64	10		29.69	29.39	1	
	53	9		58	28	2	
30.71	41	8		47	17	3	
59	29	7		36	06	4	
48	18	6		25	28.95	5	
37	07	5	lower.	14	84	6	higher.
25	29.95	4		03	73	7	
14	84	3		28.92	62	8	
03	73	2		81	51	9	
29.91	61	1		70	—	10	
80	50	0		59	—	11	

To make uſe of this table, ſeek the height which the barometer is found to ſtand at in the left-hand column, if the boiling point is adjuſted either in the firſt or third method, and in the ſecond column if it is adjuſted in the ſecond method; the correſponding number in the third column ſhews how much the point of 212° muſt be placed above or below the obſerved point, expreſſed in thouſandth parts of the interval between the boiling and freezing point: for example, ſuppoſe the boiling point is

is adjuſted in ſteam when the barometer is at 29 inches, and that the interval between the boiling and freezing points is 11 inches; the neareſt number to 29 in the left-hand column is 29.03, and the correſponding number in the table is 7 higher, and therefore the mark of 212° muſt be placed higher than the obſerved point by $\frac{7}{1000}$ of the interval between boiling and freezing, that is, by $\frac{11 \times 7}{1000}$, or .077 of an inch.

This method of correcting the boiling point is not ſtrictly juſt, unleſs the tube is of an equal bore in all its parts; but the tube is very ſeldom ſo much unequal as to cauſe any ſenſible error, where the whole correction is ſo ſmall. The trouble of making the correction will be abridged by making a diagonal ſcale ſuch as is repreſented in fig. 5.

It is not very material what kind of water is uſed for adjuſting the boiling point, ſo that it is not at all ſalt; only, if any kind of hard water is uſed, it is better that it ſhould be kept boiling for at leaſt ten minutes before it is uſed. But we would adviſe all thoſe deſirous of adjuſting thermometers in the moſt accurate manner for nice experiments, to employ rain or diſtilled water, and to perform the operation in the firſt mentioned manner, that is, in ſteam.

On

On the freezing point.

In adjufting the freezing as well as the boiling point, the quickfilver in the tube ought to be kept of the fame heat as that in the ball. In the generality of thermometers, indeed, the diftance of the freezing point from the ball is fo fmall, that the greateft error which can arife from neglecting this precaution is not very confiderable, unlefs the weather is warmer than ufual; but as the freezing point is frequently placed at a confiderable diftance from the ball, the operator fhould always be careful either to pile the pounded ice to fuch a height above the ball, that the error, which can arife from the quickfilver in the remaining part of the tube not being heated equally with that in the ball, fhall be very fmall; or he muft correct the obferved point, upon that account, according to the following table:

Heat of the air.	Correction.
42°	.00087
52	.00174
62	.00261
72	.00348
82	.00435

The

The first column of this table is the heat of the air, and the second is the correction expressed in 1000th parts of the distance between the freezing point and the surface of the ice: for example, if the freezing point stands seven inches above the surface of the ice, and the heat of the room is 62, the point of 32° should be placed $7 \times .00261$, or .018 of an inch lower than the observed point. This correction also would be made more easy by the help of a diagonal scale, similar to that proposed for the boiling point.

On the precautions necessary to be observed in making observations with thermometers.

In trying the heat of liquors care should be taken that the quicksilver in the tube of the thermometer be heated to the same degree as that in the ball; or, if this cannot be done conveniently, the observed heat should be corrected on that account: but for this we refer to the former part, p. 835.

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TABLES.

V.

W.

The End of the SIXTY-SEVENTH VOLUME.

The Number of PLATES *in this Volume is* TWENTY.

The ſecond, containing two different ſubjects, is marked Tab. II. at the Top, and Tab. III. below. The drawing of the eleventh Plate, having been ſent in too late to be numbered in the regular ſeries, is marked Tab. X*.

The letter-preſs tables ſhould be bound in upon guards by the middle, to avoid any folding out; and in thoſe which muſt be looked at ſide-ways, the page ſhould lie towards the right-hand.

ERRATA.

ERRATA.

Page	Line	
58,	9.	*for* commmunicate, *read* communicate,
85,	15.	*for* XLVIII, *read* LIV.
128,	4.	from the bottom, for "and not all" *read* "and not at all"
131,	16 and 17.	*for* (as the millers term it when no Iron is concerned) *read* (as the millers term it) where no iron is concerned
162,	6.	*for* Satellites, *read* Satellite.
165,	9.	*for* ineptats, *read* ineptas
258,	3.	from the bottom, *for* but, *read* long
258,	2.	from the botttom, *for* long, *read* but
354,	2.	*for* the year 1775, *read* the year 1776.
475,	13.	*for* creduiitity, *read* credulity
518,	7.	from the bottom, *for* $\frac{1}{2000}$ *read* $\frac{1}{20,000}$.
519,	7.	*for* 233°, 54′, 15″ *read* 233°, 53′. 15″
520,	4.	*insert* ∠ c by 4th observation = 9°, 59′, 0″ — 9°, 38′, 15″
521,	2.	*for* mountains, *read* mountain.
522,	2.	*for* correct for the signal 59″, *read* 54″
530,	5.	*for* 27,7025, *read* 25,7025
541,	4.	*for* above at C. *read* above at B.
545,	11.	*for* correct height in fathom 686,619, *read* 685,619
546,	8.	*for* difference of Log. 654,157, *read* 654,109
547,	11.	*for* (in p. 556), *read* (in p. 532)
556,	17.	*for* two, *read* too
560,	1.	*for* feet, *read* grains
	18.	*for* 13358,5, *read* 13558,5.
562,	12.	*for* barometer, *read* manometer
568,	5.	from the bottom, *for* $\overline{T-S}\times\overline{E-e-x}=S-x$, *read* $\frac{\overline{T-S}\times\overline{E-e-x}}{E}=S-x$.
569,	19.	*dele* the semicolon after quantity, and insert it after instance
578,	5.	from the bottom, *for* the attached Therm. *read* the two attached Therm.
585,	2.	*read,* see p. 574 and 567
		in the column for 25 inches, and against 21 *for* 53,2, *read* 53,1
586,	3.	*add,* see p. 568 and 569
		In the 4th col. of the table at the top, *for* 16,10, *read* 15,10.

Page Line

587, in the 2d col. against 19,90, *for* 12337,0, *read* 12377,8

20,70, *for* 11350,0, *read* 11350,8.

588, 30,40, *for* 1236,6, *read* 1336,6

in the last col. *for* 81,0, *read* 81,8.

597, 2. *for* uppermost, *read* approximate

In plate XI. Ist col. of the table of the ∠' and sides of the △' for *End of the base* 1. 2. read *End, of the base,* 1. 2.

In the small scale of feet at the side of the section of the mountains for 1500, *read* 15000 feet.

608, 3. *for* July, *read* August

628, 4. from the bottom *for* 159, *read* 152.

642, 13. *for* pistons were, *read* piston was

658, 10. *for* plate XVI. *read* plate XV.

662, 5. *for* plate XVII. *read* plate XVI.

683, 15. *after* inverse, *insert* ratio,

685, 5. a point *after* unaltered. *And for* but that in, *read* But in.

705, Tab. IV. 9th experiment, column 132° to 152°, *for* 9,14350, *read* 9,14550.

736, At the end of the note, *for* Barometer, *read* Barometers

777, 1774. December 10, weight of quicksilver to air, *for* 11445, *read* 10445.

781, 13th Station, observed superior barometer, *for* 25,691, *read* 25,961

785, 3d Observation at Belmont Castle, inferior equated barometer, *for* 29, 64. *read* 29,664

819, 7. *for* were the short, *read* were short

821, 5. *for* 15 *read* 16.